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A
NEW VIEW
OF
THE NATURE
OF
INFLAMMATION,

WITH CASES OF
CROUP AND BRONCHITIS,
ILLUSTRATING A SIMPLE AND SUCCESSFUL
MODE OF TREATMENT.

AND OF THE
USE OF THE THYMUS GLAND, &c.

By WILLIAM FORRESTER BOW, M. D.

EDINBURGH:
JOHN HAMILTON, 15, SOUTH ST. ANDREW STREET;
W. DAVISON, ALNWICK; AND E. CHARNLEY,
NEWCASTLE UPON TYNE.

1834.

Printed by W. Davison, Alnwick.

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A NEW VIEW

OF THE

NATURE OF INFLAMMATION, &c.

THE mode of cure recommended in *Cynanche Trachealis* and *Bronchitis* seems, by general consent, to be so firmly established that I fear, as I at once declare myself opposed to the practice and also to the principles on which it is founded, few may trouble themselves to follow me further than this declaration. I have reason to apprehend so from the fact that, although debility of the capillaries of an inflamed part be now very generally admitted, fears are entertained lest the inferences which may arise out of the doctrine should lead to deviation from what is considered unerring practice.

Such apprehensions can only have originated from the supposition that we have arrived at the acme of perfection in the treatment of inflammatory diseases, and that any deviation suggested by the advocates of the new doctrine must necessarily be to the disadvantage of the patient; but the mortality from these diseases, even when treated by the ablest men, nullifies our claim to any such perfection.

The great argument in support of arterial excitement in inflammatory attacks is drawn from the relief obtained by blood-letting; but this argument is fallacious and undetected as such, because the *modus operandi* of the remedy, being presumed to be perfectly understood, has never been questioned. On this point, in the proper place, I shall attempt to explain myself.

No material change in the treatment has as yet followed the important discovery of the true pathological condition of the blood-vessels of an inflamed part, therefore, the fears alluded to have hitherto proved groundless; but that no change has taken place is, in my humble opinion, less owing to the result of our boasted treatment than to the doctrine of debility not having been carried far enough. I propose to extend that doctrine, and, if the reader will but accompany me to the end, I trust, that if he find I have little else than observation to offer as the foundation of my theoretical views, he will acknowledge that the result of the mode of cure to which they have given origin promises no mean support in favour of their soundness.

That there is a loss of contractile power in the vascular structure of an inflamed part cannot be denied, the wonder is that there ever should have been a doubt about it. It is said, however, that the debility of the capillaries is followed by an increased action of the larger arteries leading to the inflamed part; that is, that the larger arteries, so far from partaking of the debility which affects their terminations or smaller arteries, gain an increase in their contractile power. I may be incorrect in thus stating the supposed condition of the larger arteries, but I think such is the meaning of Dr. Philip, for he says, "inflammation is terminated by resolution when the capillaries are so far excited and the larger arteries so far weakened by the preternatural action of the latter, that the power of the capillaries is again in due proportion to the *vis a tergo*."

The action of the heart and arteries is contractile; they possess the power of contracting on their contents, and one would think the greater the power the more complete would be the contractions. Now, if debility of the smaller vessels be followed by an increased action of the heart and larger arteries, as is supposed to be the case in sympathetic fever, we ought to have a pulse less frequent than usual; for as the nature of arterial action is contractile, the contraction must be more complete if this action be increased, and as the time required for a more perfect contraction must be greater, there must consequently be fewer in a given time, supposing the measure of each diastole to continue unaltered or nearly so.

I presume it will not be denied that in sympathetic fever the balance of the circulation is lost, there being a greater quantity of blood than natural in all the arteries of the body; how then, may I ask, can more vigorous contractions enable the arterial system to contain this increased quantity of blood? This argument I borrow from Dr. Philip, who, speaking of the congestion of the small vessels of an inflamed part, says, "although we are assured that this symptom, which can only depend on an increased quantity of blood in the vessels, arises from their increased action, it would be impossible to shew how this could happen; how a more vigorous contraction of the vessels can enable them to receive a greater quantity of blood." The argument is strong and conclusive, whether applied to the congestion of the smaller, or the distention of the larger, vessels.

Instead then of increased action of the larger arteries, I would say (in so far as the blood-vessels are concerned) that inflammation consists in debility of the vessels of the part inflamed, followed by weakened action of the arteries leading to that part; and that when sympathetic fever is formed, the contractile power of the heart itself, and of the arterial system generally, is defective.

In this view of the condition of the larger arteries in inflammation I am supported by the opinion of Dr. Billing. I say supported, for I am not indebted to him for it. His work (First Principles of Medicine) was published in 1831; mine, on the Nature of Fever, in 1829, in which I express myself as follows:—"Perhaps it may be difficult to conceive that the heart and arteries should, during febrile excitement, be deficient in contractility or the power with which they contract, seeing that the vascular commotion is frequently very great; nevertheless it is true."

In sympathetic fever there is a full pulse denoting a free dilatation of the artery, and there is a quick pulse denoting frequency in contractile effort; but if each contraction were perfected, it is evident that no such quickness could obtain. A quick and full pulse betokens a want of contractile power in the heart and arteries. A quick and feeble pulse denotes increased contractile power in the heart and arteries; the velocity of the blood in the arteries is increased, but its volume is diminished, for no sooner is the attempt at dilatation made than the contraction is renewed; therefore these contractions are in quick succession. I would say, that a quick and full pulse was the effect of frequent but unavailing efforts of the heart to empty itself by complete contractions, and that a quick and feeble pulse was the effect of frequent but unavailing attempts at dilatation. The slow and feeble pulse is also the effect of powerful contraction.

The disturbance of the vascular system in inflammation has too long diverted attention from the part which the nerves perform in the process. Indeed, it strikes me that the brilliant discovery of the circulation of the blood, however much it may have advanced our physiological and pathological knowledge, has in some respects most certainly retarded it. That step gained, further progress has been slow; for the discovery, it was thought, would furnish a key to all the functions, and thereupon the blood-vessels, the mere carriers of the pabulum of life, were endowed with attributes, of which time has not yet divested them. There is no process carried on in the system, the explanation of which has not become fettered by their presumed ruling agency, and, in the morbid one now before us, they are still considered to be the first aggressors in the deviation from healthy action, instead of their altered condition being regarded as but a secondary effect of the remote cause.

In the Medico-Chirurgical Review for October 1832, the reviewer of Mr. James' work says, "in acute phlegmonous inflammation not only is the pain pungent, but the ordinary sensibility of the part is excessively increased. Now we know that such an increase argues an augmentation of the powers of the nervous system. But such an augmentation is hardly consistent with debility in the vascular system, and what is applicable to the two systems considered each as a whole, seems fairly applicable to their parts." According to my own observation, there can be no augmentation of the powers of the nervous system as a whole beyond that which it possesses in the healthy state of the body: for nervous influence, however generated, is so slowly generated that its supply is never equal to the expenditure, unless, indeed, it be during the temporary cessation of some functions, as in sleep. But, although there cannot be an augmentation of the powers of the nervous system considered as a whole, there may be of a part of that system, as is the case with the sentient nerves of an inflamed part, which increase of power is derived or withheld from other nerves whose functions consequently become impaired.

That one nerve or set of nerves cannot become excited unless at the expense of some other nerve or set of nerves, I believe to be a law of nervous action, which I doubt not will one day become satisfactorily established. This law is in constant operation; some of the phenomena to which it gives rise have not escaped observation, nor could they, for there is not a moment of our existence in which they do not present themselves.

The act of blushing, so differently explained to suit particular views, is not only an instance of the operation of this law, but it likewise shews the rapidity with which the effect follows the cause. And does not this disturbance of equilibrium in the distribution of the nervous influence of the cheek, resemble the disturbance of the equilibrium of the electric fluid in inanimate bodies? If a glass rod be rubbed with silk, a portion of electricity leaves the silk and enters the glass, it becomes positive, and the silk negative. So, with regard to blushing, the cause for it no sooner arises than certain nerves become excited (positive) at the expense of others conveying contractile power to the neighbouring capillary vessels. These vessels therefore become weakened (negative) and permit of distention by red blood, giving rise to the suffusion of the cheek. The cause of the excitement being removed, the equilibrium in the distribution of nervous influence is restored, and the vessels contract as formerly. A well-marked effect of this law I have repeatedly witnessed in new-born infants. When the urine passes for the first time through the canal of the urethra, it produces excitement of the sentient nerves of the lining membrane; this excitement is sustained at the expense of the nerves conveying contractility to the neighbouring vascular structure, hence distention of that structure follows and priapism results.

Instead of augmentation of the powers of the nervous system being hardly consistent with debility in the vascular system, I hold that excitement of the sentient nerves or any nerves, other than those conveying contractility to the vascular structure, is not only incompatible with increased vascular action, but is, and must be, a cause of vascular debility.

If, by violent exercise, I raise my pulse from eighty to a hundred and twenty, and at the same time feel almost every artery of the body throb, I am told that by muscular exertion I have stimulated the heart and arteries to immoderate action; but I do not think that such is the fact. I rather believe that, to sustain the excitement of the motor nerves, the arterial system becomes robbed of its due force of nervous influence, consequently of its contractile power, and therefore becomes distended to inconvenience. What is called immoderate action is a state produced by the loss of power to act forcibly; in this state the heart cannot empty itself; its attempts to do so are frequent but unavailing. As soon, however, as the muscular exertion ceases, the stream of nervous influence being no longer diverted from the arterial system, it begins to recover its contractility and in a short time is enabled to unload itself.

The reviewer alluded to says, "in growth few will pretend to say the vessels are weakened, for they are then most active, yet in inflammation, analogically comparable to growth, we are told that they are so." I, for one, believe that the contractile force of the arterial system is deficient during the growth of the body, compared to what it is after growth, and that this state is not only the consequence of the activity or excitement of the nerves employed in the formative processes, but that this state, by presenting a greater volume of blood to be operated on by these nerves, is necessary to growth. I agree with the reviewer that growth is an active process, but the activity is nervous, not vascular. So far from its being an active vascular process, that agency, which alone could give the heart and arteries vigour, is withheld, in obedience to the law I have spoken of, to sustain the nervous activity. But, as the period of growth advances and approaches to an end, the necessity for nervous activity gradually lessens, and as gradually then does the arterial system gain power, marked by less and less numerous, but more perfect, contractions.

To explain, in the best way I can, my idea of this law of nervous action, which I think does truly obtain, I shall suppose a pond fed by a stream just large enough to provide a force of water capable of turning five or six mill-wheels, emitted by as many separate sluices. If one of these sluices be enlarged, the quantity of water there discharged will of course be increased, and the wheel be made to revolve more rapidly. But it is evident that the increased supply of water which gives the additional activity to this one wheel must be furnished at the expense of the others, consequently their motions must become proportionably sluggish. So, with regard to the animal frame, if a function, digestion for example, proceed with activity, the nervous influence necessary to effect this process is furnished at the expense of parts whose functions, for the time, become imperfectly or slowly performed. It is said that during digestion the heart is stimulated to increased action, as denoted by a more rapid and fuller pulse; but the fact is it yields a portion of its nervous influence to sustain the process going on in the stomach, and, therefore, its contractions become somewhat more rapid but far less complete.

The mills may be for different purposes, but it is the same power which sets each in motion; so in the animal frame there are various functions effected by one power, nervous influence; the difference of function depending on the modification of nervous matter.

If, instead of one of the sluices being enlarged, one or more of them be closed, it is plain that the water will not only accumulate in the pond, but that, owing to the increase of weight, a greater quantity will be forced through the remaining open sluices in a given time. So is it with nervous influence; if one or more of the functions be suspended, as during sleep, others will become more active. The heart acts with more energy during sleep; that is, it gains in contractile force, by which its injecting power is greatly increased; and as its contractions become more complete there are fewer of them in a minute.

Some physiologists imagine that, because the contractions of the heart become reduced in number during sleep, its action is necessarily weaker, and even assign a reason for this weakness. Professor Alison says, "the action of the heart becomes somewhat slower and perhaps somewhat weaker during sleep, probably merely from the want of the stimulating influence of sensations and voluntary exertions." Had it been said that the *pulse* became somewhat weaker, all would have been right; for the heart and larger arteries having their contractile power augmented during sleep, the velocity of the stream of blood is increased but its volume is diminished, and, therefore, the pulse becomes somewhat less perceptible. With regard to the influence of voluntary exertion over the heart's action, I have already stated what I think of it, and may here observe that the suspension of volition, so far from exerting a weakening influence on the heart's action during sleep, is a cause of its increased vigour.

The contractility of the heart is augmented in sleep, because this organ is then abundantly supplied with nervous influence, owing to the suspension of sensation and voluntary motion. During the lethargy of hybernating animals, the contractility of the heart is further augmented by the almost total suspension of respiration, so much so indeed, as is shewn by the able experiments of Dr. Marshall Hall, that the stimulus of arterial blood is not required to insure the continued flow of nervous influence towards it, as is the case when the animal is roused from its lethargy. Were it not then for the stimulus of arterial blood, the resumption of the suspended functions would deprive the heart of the power of acting.

The minute arteries, in childhood or during the period of growth of the body, do not gain in contractile power during sleep, for the nerves which effect the chemical changes are then most active, and their excitement is sustained at the expense of these vessels. This condition of the arterial system,—viz. the increased injecting power of the heart and larger arteries and the comparatively diminished contractility of the minuter vessels,—is explanatory enough of some of the phenomena to which it gives rise. For example, the flushed face of a healthy child when sleeping profoundly, and in the same child, priapism, and expanded iris.

This state of the iris led Professor Mackenzie of Glasgow to believe that the natural state of the pupil "is closure, not expansion." He came to the conclusion that this is its natural state from the fact, discovered by Fontana, that during sleep the iris is expanded. "Approach a *child* fast asleep," says he, "and suddenly raise the upper eye-lid, the pupil will appear all but closed." The truth of Fontana's discovery, Mr. Mackenzie says, has been acknowledged by Janin and Cuvier. I have access only to the latter authority, and there the fact is merely stated without adverting to age. In an essay * on the causes which affect the size of the pupil, by Nicholas Littleton, Esq., I find the following:—"One cause of diminished pupil, which I believe has been hitherto unknown, is sleep. During this state of the body, the pupil will be found at, perhaps, its extreme of diminution, especially when the sleep is profound. I have remarked it at various ages and in different diseases, and have no doubt that it is always the case in all instances where the light is capable of affecting the pupil during the waking state." Although Mr. Littleton has observed the circumstance at various ages, I suspect that in advanced or even mature age, the iris will not be found expanded, unless, indeed, digestion be proceeding actively at the same time.

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* London Medical and Physical Journal, August 1816.

I think, with Haller, that the iris is an erectile membrane, and its state during sleep, in childhood, I would offer as an argument that it is so; that is, if analogy have any weight, for, according to my own observation, priapism is a concomitant of sound sleep in childhood.

Mr. Mackenzie was at a loss to explain the closure of the pupil in *iritis*, and therefore he ascribed it to "the fact that the *natural* state of that *opening* (the state into which it falls during the insensibility of sleep) is *closure*, not *expansion*. Thus then it would appear that the iris acquires its natural state when it becomes morbidly affected. The expansion of the iris when inflamed is caused by the distention of its weakened vessels; its blood-vessels acquire the condition of other blood-vessels in other inflamed parts.

Before I attempt, with the assistance of this law of nervous action, to explain the phenomena of inflammation, I shall, by way of further exemplifying its operation, take the opportunity of correcting an error in my view of the pathology of the arterial system during the first stage of fever, which, since I published my "Notions of the Nature of Fever," a more attentive observation of nervous action has pointed out. The immediate effect of the remote cause of fever I said was impaired energy of the nervous system, and, as a consequence of this general impairment, I conceived the heart and arteries to be in a state of debility. Had I confined this impairment of energy to the brain and spinal marrow, instead of giving it to the nervous system generally, in all likelihood I would have escaped a blunder which cost me much trouble; for at the time I felt there was something wrong, something which embarrassed me greatly, yet I could not detect it. Instead of debility of the arterial system being caused by impaired energy of the brain during the cold stage of fever, the contractile power of the heart and arteries is morbidly augmented. The nerves which convey to them their power of acting, proceeding from the sympathetic system, acquire a positive state in consequence of an opposite state, produced by the operation of the remote cause, in the brain and spinal marrow, and of course in the nerves proceeding from these centres. The feeble pulse is not to be attributed to weakened action of the heart, but to its having gained an increased power of acting; its contractions are effected powerfully and follow each other rapidly, with a very imperfect diastole intervening, so that it can receive and transmit but little blood. The proper change of the blood in the lungs is not perfected, therefore, the fluid circulating in the arteries approaches the state of venous blood, consequently, and fortunately, is deficient in its stimulating property. In this state, the heart requires no stimulus from its contents, for, like that of animals during hybernation, nervous influence is forced upon it. If, in this state, blood duly decarbonized were to enter the heart, the probability is that the contraction of the left ventricle would be so powerful and continued as to interrupt the circulation. Dr. Hall thinks that, as the heart continues to act with black blood circulating during hybernation, the ventricle changes its nature and becomes veno-contractile. Its nature is so far changed that, whilst all the functions are in play, it requires the stimulus of arterial blood to attract towards it the power of acting, whereas, when some of the functions are suspended, this power is determined to it, independently of such stimulus; it would contract, for a time, equally well, upon lukewarm water.

The imperfect products secreted during the first stage of fever at length irritate the extremities of the sentient nerves, as would foreign matter introduced from without, and thus produce reaction, the second stage.

Impressed with the idea that debility was the cause of the condition of the arterial system in the cold stage of fever, I naturally enough conceived that as there is a similar state, differing only in degree, in cholera, that this too originated in debility. Having, as I presume, discovered my mistake, I shall here state in as few words as possible what my ideas are of the nature of the morbid process which gives rise to the symptoms that characterize cholera, and especially as most of these symptoms may be traced to the operation of the law of nervous action I wish to make apparent.

I agree with those pathologists who believe the disease to consist in a failure in power of the sympathetic system of nerves, in that division only, however, which is supposed to effect the chemical changes which are constantly carried on in the body.

Many physiologists, amongst whom are Lobstein and Broussais, regard the sympathetic nerve as presiding over nutrition in its extensive signification. Johnstone conceived that the involuntary muscles owed their power of motion to the ganglionic system. In time, I doubt not, it will be proved that they are all in part right; that as the spinal nerves effect voluntary motion and sensation, so will it be found that the sympathetics effect the involuntary movements and the chemical changes which constantly proceed. Some of the secretions are not constantly produced, and in their production the sympathetics are little or not at all concerned; these secretions are effected through the agency of the *par vagum.*

The sympathetic nerves effect those changes which are necessary to the growth and repair of the body,—those secretions that are perpetually required, such as the fluids lubricating serous membranes and most of the mucous membranes,—and the excretions which are perpetually separated, such as the urine. The office of the *par vagum* is to effect those secretions the production of which is more particularly influenced by external causes, such as the mucus of the air tubes, the gastric fluid, &c., and those which are only occasionally required, as the milk.

If the remote cause of cholera act by producing a negative state of that division of the great sympathetic which effects chemical changes, it must follow, according to my view, that the other division, which effects the involuntary movements, will immediately assume a positive state. This, apparently, does happen, and from these opposite states of the two divisions of the sympathetic system arise those symptoms which are considered characteristic of the disease. The non-production of some of the secretions is indicative enough of failure in the secreting division; and of the excited or positive state of the other division, we have ample proof in the powerful spasmodic contractions of all the muscles of involuntary motion. The heart and arteries labour under excess of power; the dilatations of the ventricle become so imperfect that it can receive but little blood, and, although it propels it with vigour, the quantity is so small, and the receiving arteries so constricted, that perceptible impulsion does not extend. If death take place during the collapse, the walls of the left ventricle are, according to Dr. Craigie, always firm and hard, and the capacity very small, as if the ventricle had been much and forcibly contracted. The intestines, both great and small, are thrown into violent contractions, and after death they are found very contracted. The bladder has obtained the notice of every one, owing to the rigidity it acquires from violent contraction.

Some of the secretions are more implicated than others in this disease, but those only which are effected solely by the agency of the sympathetics are totally arrested; and this proves cholera to be essentially an affection of that system of nerves.

In cholera there is a total suppression of the secretion of urine, but if the patient be a nurse the secretion of milk is increased. This secretion is not arrested, because it is not effected by the sympathetics, but by the *par vagum.* The quantity of milk is increased, because the secreting power of the *par vagum* is augmented by a greater supply of nervous influence being determined to them in consequence of the suspended expenditure of it by the secreting division of the sympathetics. The secretion of urine is totally suppressed, because it is effected entirely by the sympathetics: "the kidneys in particular receive no nerves from the brain or spinal marrow;" * "the filaments of the great sympathetic alone are distributed to the kidneys." †

It has been remarked as wonderful, that the heart should continue to beat so long as it frequently does in the collapsed stage, supplied all the while with so little blood, and that little any thing but blood. The fact is, the heart is not then influenced by its contents, provided they be neither directly sedative nor stimulant. In health, the stimulus of arterial blood is necessary to insure that supply of nervous influence which either constitutes or excites its contractility; but, in the morbid state before us, this nervous influence is forced upon it, owing to an immense drain on the general expenditure being stopped by the suspension of the secreting function of the sympathetic. In the healthy subject we would destroy life were we to inject the heart, through the veins, with but an ounce of water; for, during the occupation of its cavity by the water, the natural and necessary

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* Richerand. † Hooper's Medical Dictionary.

The author must differ from these authorities, for he has seen, in the fœtus, nerves pass directly from the spinal marrow to the kidneys; however, these nerves have nothing to do with the function of the organs, except to furnish them with the necessary sensibility.

stimulus being withdrawn, its nervous influence would be attracted elsewhere, and its contractions would cease. In the collapsed stage of cholera, however, gallons of saltish water have been injected, apparently without detriment, proving, what I assert, that the contractions of the heart in this condition, and of course in similar morbid states, are, to a certain extent, not influenced by what it may contain. On the principle of diminishing the force of the heart's action in cholera, a watery solution of opium injected by the veins might prove beneficial: so administered in tetanus it has proved successful.

During an attack of cholera there are some phenomena which occasionally present themselves, and which have been considered rather of a negative than of a symptomatic character; to these I might advert as arising from the operation of the said law of nervous action, but, as I may have to notice some instances of its operation in other affections, I must pass them over. Yet, before quitting the subject, I may as well glance at the old and shrivelled appearance exhibited by those in the stage of collapse. They appear to be sinking from a sudden attack of old age, and, in reality, die the death of old age. According to nature, the same change is worked in years which in cholera is accomplished in hours. As old age advances upon man, the arterial system gradually gains in contractility in proportion to the natural gradual failure in the functions of the chemical division of the sympathetic. In cholera there is a sudden and general failure in the function of the same nerve, and, as a necessary consequence, the contractility of the arterial system is as suddenly augmented to the degree of forcing the whole blood of the body into the venous system.

With regard to the morbid process of inflammation, the first effect of the remote cause is excitement of the sentient nerves of the part, which, according to the law of nervous action alluded to, cannot become established unless it be at the expense of other nerves,—of those which convey contractility to the neighbouring vascular structure and of those which effect the secretions of the part. The vascular structure, being thus deprived of its contractile power, permits of distention; vessels which before carried colourless fluid only, now admit red globules, and all the vessels of the part becoming congested, we have the indisputable consequences, redness and swelling. The congested vessels, by pressing and stretching nervous fibrils not originally irritated by the first remote cause, soon become a cause of extending the excitement, and this additional excitement, in its turn, extends the congestion. Cause and effect thus proceed spreading the mischief until remoter parts are involved. The larger arteries leading to the inflamed part yield their nervous influence and become distended. The products of secretion throughout the body become more or less defective, presenting another source of irritation. The nerves of the skin, owing to their exquisite sensibility, readily become excited by its impaired secretion, and, as this new excitement proceeds, the arterial system, with the heart itself, is deprived more and more of its nervous influence; general arterial distention follows, and sympathetic fever is the result.

As local inflammation consists in an excited state of the sentient nerves of the part co-existing with or producing an opposite or negative condition of the nerves which effect vascular contractility and the secretions of the part, so do I conceive the fever of inflammation to consist in excitement of the *system* of sentient nerves, producing impaired contractility of the arterial system.

Perhaps this fever may be strictly sympathetic, that the system of sentient nerves becomes gradually excited without the interposition of impaired secretion of the skin, as I have it; but I am induced to look upon defective secretion as the cause of the general excitement, for I believe it is the irritation from the defective products of secretion in the cold stage of common fever which is the cause of the second stage.

The condition of the arterial system in sympathetic fever, laying aside the local affection, is analogous to that produced by violent exercise, and this state in both cases is ascribable to the same cause, viz.—nervous excitement. But in the one case it is the system of sentient nerves which is under excitement, in the other case it is the system of motor nerves.

In the same way I have no hesitation in admitting the analogy between growth and inflammation, that is, that the vessels during growth are in a state approaching to the condition they attain in inflammation. In both cases the debility of the vessels is produced by nervous excitement; but, during growth, the nerves which effect the process are under excitement, and not the sentient nerves, as in inflammation. The analogy between the healthy process of growth or repair, and the morbid one of inflammation, cannot be carried one step further than as regards the approaching condition of the vascular structure. Unfortunately in this weak and partial analogy has originated the doctrine that these two processes differ only in degree of activity; that the morbid process of inflammation, which every day cuts off numbers of our race, is but a more energetic illustration of the salutary process by which growth and repair are effected! I have more than once seen mischief accrue from the well-meant but mistaken practice of assisting nature, when *healthy inflammation* was supposed to be not sufficiently active; and this mischief was accounted for on the ground that we had overstepped the line and produced a too active inflammation. Whereas, in fact, we had by irritation excited the sentient nerves at the expense of the very nerves which were effecting, though as we thought too slowly, the object we had in view.

What is termed healthy inflammation in a part, consists in excitement of the nerves which effect the changes from which result growth or repair; this excitement being sustained at the expense of the nerves which convey contractile power to the vessels carrying the blood to be acted on. The vessels, forced to yield their nervous influence to support the activity of these chemical nerves, are thus enabled to present a greater volume of blood to be decomposed than they otherwise could. True inflammation consists in excitement of the *sentient* nerves, and therefore pain is a necessary and prominent symptom; whereas pain is not a necessary attendant on the healthy kind; for the nerves then under excitement belong to the sympathetic system.

Blood-letting in the cure of inflammation is deemed indispensable by all parties; by the advocates for increased action and by the advocates for diminished action. Blood-letting, by the one party, is supposed to act by relaxing the large vessels; according to the other, by relaxing both the large and the small. And indeed the success of this remedy is brought forward by both parties as proof of the soundness of the doctrine of increased action of the larger vessels, and by one party of increased action of the smaller vessels also.

As the supposed *modus operandi* of venesection is appealed to as an incontrovertible argument in support of increased action of the arteries, it will of course be made the leading objection to my views, which represent the condition of the arterial system, in inflammation and its fever, to be that of debility from the capillaries to the heart itself. I acknowledge the curative agency of blood-letting, but I cannot grant that it operates by relaxing or debilitating the arteries, for I have said that their defect is relaxation or debility.

Weighty as the objection may at present appear, I trust that, by pointing out the true mode in which blood-letting operates in effecting a cure, I shall, rather than otherwise, gain an argument in support of my views; and, if so, exculpate myself from the seeming arrogancy with which I set out, asserting, as I did, that the argument deduced from blood-letting was fallacious.

With regard to venesection then; whoever has employed it frequently must, of himself, have discovered that its beneficial effects follow not on the quantity drawn, but on the effect produced on the brain by the loss of it. Some writers talk of bleeding until an impression be made on the disease, but the impression to be made is on the brain, and it is evinced by syncope or a state approaching to it. Physicians desire this effect so much, that, regardless of quantity, they frequently prescribe venesection *ad syncopen*, and, to further this end, practitioners prefer taking blood from the patient placed in the erect posture. If blood-letting acted merely by allaying sanguiferous excitement, or by emptying and relaxing the large vessels and the small, we could at all times direct the proper quantity to be taken. Blood-letting, however, in the cure of inflammation, is but the means of gaining an event favourable to the solution of the disease, which event, when brought about by other means, proves equally productive of benefit, perhaps more so. The pain of venesection, or the sight of the blood or of the lancet, frequently produces fainting, on recovery from which the local disease is often observed to have disappeared. This condition, syncope or a state approaching to it, is the event gained by blood-letting, which is favourable to the solution of the disease, and that it should be so will not be wondered at, if upon investigation it be found that the pathological condition of the arterial system during syncope is the reverse of what it is in inflammatory fever.

In inflammatory fever, from want of power to contract fully, the heart and arteries become distended; the pulsations are rapid, but the contractions of the heart are incomplete. In syncope, or a state approaching to it, the pulsations are very feeble, because the contractions of the heart are complete, and the dilatations almost imperceptible. This feebleness of pulse, however, does not denote debility or loss of power in the heart; on the contrary, it denotes augmented contractile power. In this state of the heart it often happens that we have a rapid pulse, because, although there cannot be a complete diastole, the attempts at dilatation are frequent, but as soon as the attempt is made, there is a renewal of the contraction.

In inflammatory fever, from deficient contractile power, the heart cannot empty itself. In syncope or a state approaching to it, from too great contractile power, giving a tendency to continued contraction, the heart cannot fill itself.

The brain, that it may execute its functions, requires to be regularly impinged by a certain force of blood; if it be suddenly deprived of this stimulus, its functions cease, as manifested by the abolition of those actions over which we know it presides. It has no direct influence over the heart and arteries, yet, indirectly, these are influenced by alterations in its operative condition. By the suspension of the functions of the brain, during syncope, an immense drain on the general expenditure of nervous influence is suddenly arrested. That nervous influence, not then required by the organs whose functions have ceased, may be called *free*, and is immediately attracted by, or determined to, organs whose functions are not regulated by the state of the brain. The heart and arteries, therefore, by this sudden acquisition of power, are thrown into contractions so forcible as to confine almost all the blood to the venous system. If the heart be not perfectly sound, the sudden and violent contraction upon its contents may occasion its rupture, an event not unfrequently witnessed, and generally produced by sudden syncope from moral impressions.

It has now been proved, by experiment, that the presence even of the brain is not immediately necessary to the action of the heart; some circumstances, therefore, which, formerly, appeared of easy explanation, now baffle all attempts to account for them. That a blow on the head should affect the heart, producing in it a state similar to that which it acquires in syncope, seems, to *Mr. Brodie*, so remarkable a fact, that he observes that in the history of the animal economy there are few circumstances more so: "*that an injury of a part which is not immediately essential to the heart's action, should nevertheless, under certain circumstances, have the effect of occasioning its immediate cessation.*" *

The effect of the blow is the abolition of the functions of the brain merely, immediately upon which, the heart and arterial system acquire a positive state by the acquisition of an immense force of nervous influence suddenly set *free* by the abolition of those functions.

Is it not owing to the immense force of nervous influence which is, in a manner, poured upon the heart and arteries during the lethargy of hybernating animals, from the suspension or great inactivity of some functions, that the heart continues to act, even although the stimulus of red blood, so necessary to its continued action at other times, be withheld? In syncope, the heart is similarly affected, though to a greater degree, from the same cause; it is, as it were, overpowered with nervous influence, so that its efforts at dilatation are no sooner made than the contraction is renewed: hence its action, though more powerful, is less perceptible, and therefore is said to be feeble.

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* From Paris and Fonblanque's Medical Jurisprudence.

During syncope or a state approaching to it, which, in the cure of inflammation, we produce by blood-letting, the sentient nerves become dead to the cause of their excitement, and as the arterial system was robbed of its nervous influence to support the excitement of these nerves, it now recovers that influence. In fact, the nerves which convey contractile power to the heart and arteries become excited to a degree proportionate to the failure of function produced by the impaired operative state of the brain. Thus by blood-letting we induce an almost or total suspension of some functions, that at their expense we may excite the heart and arteries to strong contractions, by which they relieve themselves of the distention their lost contractile power permitted. Therefore, instead of weakening arterial action by bleeding, we increase arterial action; instead of relaxing the vessels, we give them tone; but to effect this, the energy of the brain must be, more or less, impaired by the quantity of blood drawn, unless some of those happy circumstances which occasionally forward fainting come to our aid: hence small bleedings are generally deemed worse than useless in the cure.

Blood-letting then, in the cure of inflammation, acts by producing a negative state of the sentient nerves, (an excited state of these nerves being the essence of the disease), by which a diversion of nervous influence in favour of those conveying contractile power to the arterial system is accomplished. But the remedy, in early age, is often little better than the disease. Should we have it in our power to congratulate ourselves on having removed immediate danger, we have also frequently to regret that whilst we save life, it is at the cost of a lingering convalescence, and perhaps the robust appearance exhibited before the attack may never after be regained. By depletion we may cure the disease, but then all our care and attention are needed to combat the injurious effects of the remedy. In these injurious effects are to be found objections, which indeed are acknowledged but borne with, inasmuch as it is considered there is no alternative. And, truly, were blood-letting at all times efficacious in checking the progress of inflammation, its inconveniences might be held lightly, but when it fails so often as it does in bronchitis and croup, any other mode of cure, not followed by injurious consequences to the constitution, and giving an equal average of cures, is greatly to be desired, and certainly ought to deserve attention.

A diversion of nervous influence in favour of the arterial system is the object to be gained in the cure of inflammation and its fever, and this is often, not always, attained by venesection. But, in early life at least, there is no necessity to have recourse to such means; there is no necessity to injure the constitution, by depriving the body of the very fluid which, the local disease being removed, we would re-impart were it possible. We have the power of directly invigorating the heart and arteries by causing a diversion in their favour of nervous influence from the surface of the body; and the agents which act sedatively on the sentient nerves of the skin will be found to be the means.

In the treatment of common fever, I found that opium applied externally had the effect of allaying the nervous excitement of the second stage, by causing a diversion of nervous influence towards the heart and arteries, and thereby increasing their contractile power. That such is the action of opium, externally employed, I became convinced, both by observation and experiment, and as I thought this was all that was wanted to resolve inflammation, I determined to trust to the external application of opium, instead of venesection, in the first cases of tracheitis and bronchitis that might come under my care.

The first case, in which I had an opportunity of putting my plan to the test of experiment, was one of bronchitis after measles. The little patient had slept none during the night previous to the evening on which I saw her. The fits of coughing had been violent and frequent, and the breathing so difficult, that at times the parents were afraid of suffocation. They dreaded such another night, and therefore I was requested to prescribe. The heat of skin was great; the pulse rapid; the face flushed; the eyes as if swollen; the breathing laboured and wheezing. She could not lie in bed, but was constantly throwing her arms about, and changing her position on her mother's knee. I ordered the neck, chest, and arms to be rubbed with a strong opiate liniment. This operation was hardly over ere she requested to be put to bed. She slept soundly; and, in the morning, appeared as if nothing had been the matter: she made a hearty breakfast, and wanted to go out of doors to play.

This was a case which, I need hardly say, demanded, according to existing notions, blood-letting, and which I would have so treated, had it occurred some months earlier.

I will not say that venesection hurries the fatal catastrophe in similar cases, however much I may suspect that it does; but this I must say, that, if my views of the pathology of such cases be correct, blood-letting, even by leeches, is forbidden.

Blood-letting is employed to unload the vessels which, by being fuller than natural, impede respiration; but if we effect this, we at the same time weaken the patient, when our aim, after measles, ought to be to strengthen. By opiate friction we at once invigorate the vessels, and thereby enable them to empty themselves; we thus cut short the disease, without inflicting on the patient a lingering convalescence, which is sure to follow the cure by blood-letting.

In the spring of 1831, many well-marked cases of bronchitis occurred in this town and neighbourhood, so that I had an opportunity of testing most fully the plan I had determined to adopt. Mr. Fender, the intelligent resident Surgeon of the Alnwick Dispensary, accompanied me in my attendance on some of the following cases, and witnessed, I dare say, with not less pleasure than I did, the success of the treatment.

CASE I.

14th February, 1831.——Darling, aged nine months, was reported at noon, by Mr. Fender, to be labouring under bronchitis. I immediately proceeded with him to the patient; found the breathing difficult, the inspirations being short and frequent, accompanied with wheezing; the face very pale; the lips having a purple tinge; skin exceedingly hot; hoarseness of the voice when the child cried; pulse rapid. The child had been ill for some days, but the mother, thinking that nothing ailed it, except a common cold, did not become alarmed until this morning. Nothing had been prescribed before I saw it. Two grains of calomel were given, and the breast and back rubbed with rather more than a drachm of opiate liniment. *

The calomel was rejected from the stomach almost as soon as taken. After the application of the liniment, the child fell into a sound sleep: at two o'clock he awoke, and sucked greedily; and at our visit, a little after two, we found him in a profuse perspiration, the voice perfectly free from hoarseness, and the breathing comparatively

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* The Liniment I used was as follows; I have since added a little soap. ℞ Opii ℥j.; Linim. Camph. c. ʒviii. Digere per dies aliquot et effunde linimentum.

easy. At four o'clock the breathing had again become difficult, but, in degree, nothing equal to what it had been in the morning; the calomel and liniment were repeated. At six o'clock the child seemed calm and contented; the eyes were sprightly, and some colour had returned to the cheeks. A portion of liniment was left with the mother, with orders to apply it, should the breathing again become hurried.

15th February.—About three o'clock this morning the liniment was applied, as the mother thought the state of the breathing required it. At our visit at ten A. M. we found that the bowels had been twice moved, the effect of the calomel. The child seemed quite well, and therefore nothing was prescribed. At seven P. M. the child continued well. At the request of the mother some of the liniment was left, to be applied if necessary.

16th.—The liniment was not applied.—Cured.

CASE II.

14th February, 1831.—M. Y., aged three years and six months, for some days past has had stuffing about the chest, cough, burning skin, and great restlessness in bed during night. I saw her for the first time at nine P. M. this evening: the breathing was difficult with wheezing; the face flushed; tongue foul; pulse 120. Two grains of calomel were ordered.

15th, one P. M.—The bowels have been moved; the difficulty of breathing continues; skin hot; face still flushed; pulse about 130. A drachm and a half of liniment, and to be repeated at bed-time.

16th.—At my visit this morning, the child was at play in a neighbour's house: she had a good night, and is reported quite well.

The above case may be looked on as one of simple catarrh only, and many such I could detail, which were all as speedily cured. The liniment was not applied the first evening, because I thought I perceived a slight eruption on the neck and breast.

CASE III.

16th February, 1831.—Elizabeth Race, aged ten months. Visited this child to-day at eleven A. M.: great heat of skin; restlessness; difficult breathing, with wheezing; paleness of the face; purple hue of the lips; lividity around the mouth; pulse 160 or more, (it was difficult to count it). The mother said that the breathing had been so difficult at intervals during the last two nights, that she thought the child would have expired. A drachm and a half of liniment ordered, which Mr. Fender himself applied before we left the house.

At one o'clock the child slept soundly; pulse 120; the respirations, which, at our first visit, could not be numbered, owing to the restlessness of the little patient, were now ninety-six in the minute; skin comparatively cool. At three o'clock the child was still asleep; pulse 110; respirations 80. Liniment repeated.

17th.—The child had a good night, and is now apparently well. As she could not be kept sufficiently quiet, neither the pulse nor the respirations could be numbered. Cured.

CASE IV.

23rd February, 1831. —— Dixon, aged eight months. I saw this child at six this evening: it has had catarrhal symptoms since the 20th instant. I found the breathing hurried; the skin hot; face pale; great restlessness; pulse rapid. A drachm and a half of liniment.

24th.—After the application of the liniment, the child slept for some hours: towards midnight it awoke in a profuse perspiration. About break of day, it again fell asleep, and continued to sleep soundly until eight o'clock A. M. I saw it at half-past nine, and, as it appeared perfectly well, I did not prescribe for it.

At three P. M. I was again sent for, and was told that towards noon the child "got quite knocked down," (as the mother expressed it), since which it had got gradually worse. I found the skin extremely hot, and the breathing hurried, with wheezing; the child was quiet and drowsy, so that the pulse and respirations were easily reckoned; the former 160, the latter 84. I prescribed two grains of calomel, and a drachm and a half of liniment.

Ten P. M.—Shortly after the liniment was applied, the child became lively and playful. At present he is in a calm sleep; heat of skin less; pulse 120; respirations 70; no wheezing. Repeat the friction, and one grain of calomel.

25th, ten A. M.—The child passed a good night; the bowels have been twice moved this morning; appears lively and apparently quite well. Cured.

CASE V.

24th February, 1833.—John Robson, aged ten months, was seen by me this morning at six A. M. He had been colded and fretful for some days, but not until last night did his mother deem him seriously ill. I found the breathing laboured, with wheezing; great heat of skin; pulse rapid; face pale; colour of the lips inclining to purple. He had slept little or none during the night, and was so restless that his mother was forced to pace the room with him in her arms all that time. He frequently took the breast, but it almost always produced coughing.

I had a vial of liniment in my pocket, and saw applied rather more, I think, than three drachms of it.

Ten o'clock A. M.—About half an hour after the liniment was applied, he fell asleep: he was then put to bed, and is still sleeping. Pulse 130; inspirations, as denoted by the movement of the bed-clothes, 72; no wheezing; some colour has returned to the cheek; skin moist.

Two drachms of liniment to be applied when the child awakens, and two grains of calomel to be given.

Four o'clock P. M.—About an hour after my last visit, the liniment was applied and the calomel given, after which the child sucked greedily, and, as the mother said, seemed quite well. He again slept from twelve o'clock until three, and is now in his mother's arms apparently well. Neither pulse nor inspirations can be numbered with certainty.

Ten o'clock P. M.—The bowels have been three times moved since last visit; the skin is cool and moist; respiration perfectly free; has coughed occasionally since last visit.

A tea-spoonful of liniment to be applied to the back.

25th February.—He has slept almost the whole night, and appears quite well.

CASE VI.

29th March, 1833.—Ann Short, aged two months. I was summoned to this infant this morning, but as it was in the country, and as I could not leave the town, I did not see it. From the description of the symptoms, however, I suspected bronchitis, and prescribed two powders, each containing a grain of calomel; one to be given as soon as possible, the other two hours afterwards. A tea-spoonful of the liniment to be applied every two hours.

At ten o'clock P. M. I was again sent for; the child, I was told, was not expected to live. I hastened to the house, and, arriving there at eleven o'clock, found the cause of alarm to be that two or three times she had been seized with violent coughing, during which, as the parents expressed it, she got black in the face, her eyes becoming fixed. The pulse I could not count, it was rapid; the inspirations were irregular, but seemed to average seventy in the minute; the skin was hot; the face pale; lividity around the mouth and eyes; great restlessness. The calomel had acted on the bowels.

I caused the child to be stripped, and saw applied to her breast, bowels, and back, about half an ounce of the liniment.

The clergyman, who had been sent for to baptize the child, then performed his office; and, after waiting half an hour, we took our leave, the child being then asleep.

30th March.—The child passed a good night, and is now apparently quite well; nothing prescribed.

I happened to see this child about a month afterwards, and was astonished at its fatness, it was the fattest child I ever saw at its age, although before the attack she was considered puny.

Cases I., III., & IV. I selected from many that occurred about the same time, because there could be no dispute about their being cases of genuine bronchitis, and of a dangerous character, and these I published, with some remarks, in the sixty-sixth volume of the London Medical and Physical Journal. Having done so, I took no notes of cases which I met with afterwards, until I thought of publishing in the present form, which accounts for the long interval between Cases IV. and V. Some of the following cases of croup were also inserted in the above Journal.

CASE VII.

4th March, 1831.—William Douglas, aged two years six months, has had catarrhal symptoms for some days past: yesterday morning, his breathing was heavy and cough croupy; towards evening, he appeared worse; and, about eleven o'clock last night, the respiration became more difficult, and sonorous. I saw him for the first time at three A. M. this morning: the breathing was very difficult, and the sound emitted so loud that I heard it on approaching the house; he was extremely restless; skin hot; face flushed; pulse frequent. An emetic was given, and, after he had vomited, the breast and back were rubbed with two drachms of liniment. In half an hour he was so far relieved as to breathe with less difficulty; the respiration was not sonorous, unless when he struggled, which he did once or twice to get down from his nurse's knee. Two grains of calomel were given, and about half a drachm of lard containing five grains of opium, was rubbed on his limbs and back. Soon after he was rubbed, he became lively and playful, and then fell into a sound sleep, in which I found him at six o'clock. He perspired freely; but as his breathing was thick, though not sonorous, I awoke him, and administered two grains of calomel, with a quarter of a grain of opium.

I desired I might be called at half-past seven; the report then was, that he slept quietly and breathed easily.

At nine o'clock I saw him, and was happy to observe that the respiration was almost natural; he certainly breathed with some difficulty, but, as there was no restlessness, no heat of skin, and a pulse not at all frequent, had I seen him then for the first time, I would have thought nothing of it. Two grains of calomel.

At eleven o'clock the child was playing about the room; but, as the state of his breathing did not yet satisfy me, I ordered his neck and breast to be rubbed with two drachms of laudanum, (I could get no more of the liniment at the time); and, as I was forced to proceed to some distance, I prescribed two grains of calomel every two hours.

At eight P. M. I found him in a sound sleep; respiration perfectly free. Three of the powders had been given; the bowels had been twice moved. His nurse told me that, soon after he had been rubbed with the laudanum, he coughed a good deal, and expectorated some thickish matter; from that moment, she said, he was quite himself. The expectorated matter she did not preserve. Nothing prescribed.

5th, noon.—Continues well; the bowels have been moved since last visit.

6th, two P. M.—He is romping about the room, apparently in perfect health.

The following letter I received from Mr. Burn, Surgeon, Belford; the cases which he obligingly sent to me have an asterisk prefixed to each.

To Dr. Bow, Alnwick.

Belford, 19th July, 1831.

My dear Sir,

I have very great satisfaction in being able to state that your plan of treating croup, and other inflammatory diseases, has succeeded with me most completely. In a disease so frightful and fatal as croup, I confess I was not bold enough at first to rely altogether on the means advised by you, but, in addition, applied leeches. However, since you informed me that you found leeching rather to retard than forward the cure, I have discarded them, and I agree with you that the liniment acts with more marked effect in the cases in which they have not been applied. All my cases, in which the liniment and calomel only were used, confirm the truth of the observation you make in the London Medical and Physical Journal for this month, viz. that the change from disease to health is so rapid, that there is no convalescent period. I sincerely congratulate you on this most decided improvement in our art, and enclose a few out of many cases which have come under my care.

I am, my dear Sir, yours truly,

Robert Burn.

* CASE VIII.

25th May, 1831.—Mary Pile, aged eleven months, has been ill for thirty-six hours with difficulty of breathing, &c. The symptoms evidently becoming worse, I was sent for, and found the case one of decided croup. I ordered two drachms of liniment to be rubbed on the breast and neck, and to be repeated at intervals of six hours; also two grains and a half of calomel until the bowels were freely opened.

26th May.—The child got better and better after each application of the liniment; the bowels have been freely opened.

27th May.—Perfectly well.

* CASE IX.

16th June, 1831. —— Sutherland, aged two years six months; breathing very difficult; skin hot; pulse rapid; cough frequent, with the peculiar croupy sound.

Three drachms of liniment; two grains of calomel every hour until it operate on the bowels.

17th June.—The child was immediately relieved by the liniment, but, as the parents were prejudiced in favour of leeches, four had been applied.

19th June.—Quite well.

* CASE X.

20th June, 1831.—Mary Clarke, aged three years, has been ill twenty-four hours of a supposed cold: the case, however, is decidedly one of croup, characterized by difficult and sonorous breathing, cough, &c. Three drachms of liniment.

Nine P. M.—The child was immediately relieved by the liniment; indeed, as the mother said, before it was all applied. The breathing is again somewhat heavy, and the cough still croupy. Repeat the liniment, with a dose of calomel.

21st June.—Has slept well all night; breathing relieved; cough easier. Repeat the liniment.

22nd.—Quite well.

* CASE XI.

2nd July, 1831. —— Crinkly, aged nine years six months: a most formidable case of croup of three days standing. The difficulty of breathing most alarming; skin remarkably hot; pulse exceedingly quick. Upwards of three drachms of liniment immediately applied, and a full dose of calomel given.

3rd July.—The relief from the liniment was almost instantaneous, but as a child in the same family had died of the disease some time before, the parents could not be persuaded to trust to the liniment alone, so *they* applied six leeches, which bled freely. The calomel has acted on the bowels.

4th.—The patient continues quite well, excepting some debility occasioned by the bleeding.

* CASE XII.

11th July, 1831. —— Robson, aged two years. This case was, with regard to symptoms, exactly similar to the last: it was seen at eleven P. M., and two drachms of the liniment were ordered to be occasionally applied to the neck and breast; calomel.

12th July.—The child was immediately relieved, and slept well during the night. The bowels have acted.

13th July.—Quite well.

CASE XIII.

15th July, 1831. —— Anderson, aged two years and a half, has been ill twenty-four hours with difficult breathing and croupy cough. I saw the child at six P. M. The breathing was difficult and sonorous; skin very hot; pulse 160. As I entered the house they were about to immerse him in a hot bath; this I objected to, and saw him rubbed with more than half an ounce of liniment, and prescribed three grains of calomel.

At ten P. M. Mr. Fender saw him; his pulse then was 112; and as he slept soundly, Mr. Fender did not prescribe. The mother said that in half an hour after the application of the liniment he seemed quite well.

16th July.—The child is running out, and not to be found; he is reported, however, as quite well.

CASE XIV.

19th May, 1833. —— Robinson, aged ten months, has had symptoms of common cold for some days past. Yesterday about noon, she was seen for the first time by a medical gentleman, who prescribed calomel and a warm bath. At his visit in the evening the child was asleep, and as she appeared considerably relieved, she was not disturbed. About an hour after the visit, the child awoke, and exhibited symptoms of croup, which gradually increased in severity until two this morning, when I was called on, the medical gentleman alluded to, being unable to leave his bed, owing to a sudden attack of influenza. There was great restlessness; face flushed; pulse rapid; skin hot; cough clangous; breathing hurried, but not sonorous, unless when the child struggled to withdraw its arm from me. The bowels were twice moved whilst I was in the house. From what the messenger said who came for me, I suspected croup, and went provided with liniment. I saw half an ounce rubbed over the back, breast, and bowels. I remained half an hour with the patient, and, before leaving, the cough had become loose, and had lost its ringing sound; the difficulty of breathing was also removed, and the child seemed inclined to rest.

At 10 o'clock A. M., I found the family in high spirits; the report was, that the child had slept until eight o'clock, and then awoke as if nothing had ailed her. She appeared quite well. Nothing more prescribed.

The following case I select from some transmitted to me by Mr. Davison, Surgeon, Belford. He remarks that it was one of a most threatening aspect, and in which the good effects of the liniment were so palpable, that if he had before entertained doubts of its efficacy, the result in this case would have removed them.

CASE XV.

November 1st, 1832.—P., a girl aged three years, has been unwell for two days with cough and slight difficulty of breathing. Her parents did not become alarmed until this evening, and at nine P. M. I was sent for. The respiration, I was told, had got gradually more and more impeded, and indeed I found it most laborious; skin very hot and dry; pulse rapid; face flushed; cough dry and shrilly.

I ordered three grains of calomel to be given, and three tea-spoonfuls of liniment to be rubbed on the neck and breast.

10 o'clock P. M.—The breathing is greatly relieved, and the child, instead of being extremely restless, as was the case at my last visit, is quiet and inclined to sleep.

Repeat the liniment now, and again during the night.

November 2nd, morning.—The child has rested tolerably well during the night. The bowels have been acted on; the breathing is quite easy. Nothing prescribed.

Evening.—The breathing has again become difficult. Two grains of calomel. Two tea-spoonfuls of the liniment to be applied immediately, and occasionally during the night.

3rd November.—Has rested well during the night, and seems better.

4th November.—Quite well.

CASE XVI.

25th March, 1833.—D. Burnett, aged four months, has been suffering under symptoms of common cold since the 22nd instant. Was seen this afternoon by Mr. Fender, who reported him to be labouring under croup. I saw the child at five P. M. Mr. Fender had prescribed a grain of calomel every two hours, and the application of the liniment, which had been applied half an hour before my visit. The violence of the symptoms, I was told, had greatly abated, but, before leaving the house, I saw about three drachms of the liniment again applied.

26th March.—The child rested well during the night, and to-day appears as if nothing had ailed it.

Here I cannot refrain from quoting the very just remarks of that truly able physician Dr. Marshall Hall. He says, "of the whole number of cases of disease in infancy, a great portion occur from this inappropriate or undue application of exhausting remedies. This observation may have a salutary effect in checking the ardour of many young practitioners, who are apt to think, if they have only bled and purged and given calomel enough, they have done their duty; when, in fact, in subduing a former, they have excited a new disease, which they have not understood, and which has led to the fatal result."

Doubtless the observation will or ought to have a salutary effect in checking the ardour of the young practitioner, and, I trust, also of opening the eyes of those of maturer age to the probability, at least, that they also may go too far. Many years ago I was called to a case in which the little patient exhibited all the symptoms of pressure on the brain. The scalp had been shaved and a blister applied to the whole of its extent. The child died on the day following that on which I saw it. The history of the case was simply this: ten days before death, the patient was attacked with bronchitis, for which it was bled, leeched, purged, and blistered. Under this treatment the local disease disappeared; the patient, however, I was told, never afterwards "looked up," but gradually sunk to the state in which I found it.

Yet, after all, I do not see how the young practitioner is to act in order to avoid censure, since he is told that in all inflammatory diseases the lancet is his "anchor of hope," and, in the two diseases before us, the universal cry being, bleed, bleed!

Dr. Cheyne says that no physician of experience would refrain from blood-letting in the first stage of croup. That it is a disease to be cured, probably, in ninety-nine cases of a hundred by emetics and bleeding *timely* employed. Dr. Cheyne of course does not mean to say that that is actually the result of his practice, but that that probably would be the result were he, in every case, to see the patient in time, and that is when croup is rather threatened than established. This brings to my recollection the sentiments of a late author on croup, who says he has such horror of the disease, that he makes it a point to bleed before it exists.

In two-thirds of the cases, in country practice at least, we are not called on until the first stage is passing or has passed into the second stage, and then if we advise blood-letting, and no good effect follow, we inflict upon the patient a positive evil. For, according to Cheyne, "if bleeding fails to subdue an inflammatory disease, it will be hurtful 'by depressing the power by which the muscles act;' and on the vigorous action of the muscles which expand the chest depends a continuance of that struggle by which the lungs are supplied with air sufficient for the arterialization of the blood."

It would appear also, that, with the poorer classes of London, medical aid is not *timely* sought for, otherwise how can the great mortality in Mr. Rees' practice, in defiance of the "anchor of hope," be accounted for?

That gentleman, who is surgeon to the City Institution for Diseases of Children, declares that the first thirty cases, which came under his care after his appointment, proved fatal without a single exception, although the patients were treated according to the precepts he had received from his instructors. *

If croup be a disease to be subdued ninety-nine times out of a hundred by timely blood-letting, how comes it that medical men are constantly in search of a surer mode of treatment? In the very last number of the Medico-Chirurgical Review, that for October 1833, the editor notices two modes recommended in the works of two different authors—Dr. Walker, and Dr. Steinmelz, of Pyrmont. The former has seen many instances of relief from the repeated exhibition of lavements charged with turpentine, after leeches had been ineffectually applied to the throat. The inefficacy of the ordinary treatment by leeches and calomel induced the latter to trust almost entirely to the steady employment of emetics. The usual formula he has used is three grains of emetic tartar dissolved in an ounce of water to be sweetened with sugar, &c. The *nausea* and occasional retching should be kept up until the danger be over. "The success which Dr. S. has derived from this treatment is stated to be very gratifying."

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* Lancet, Vol. I. 1831–32, page 761.

Whether these gentlemen would have succeeded in subduing the disease sooner by taking blood from the jugular vein, I know not; but their testimony proves that, to subdue the disease, blood-letting is *not* necessary.

"It is true," says Dr. Hastings, "that, for the most part, children do not bear the loss of blood well; but in an attack which is menacing life, there is no alternative: we must adopt powerful measures, for without them the disease (bronchitis) will almost certainly prove fatal; but such is its dangerous character, that even by them its progress is often not arrested." Dr. Hastings is not blind to the injurious effects of exhausting remedies, but it would appear that we have no alternative. We must have recourse, he says, to general blood-letting to diminish the excitement of the heart and larger arteries, and to local blood-letting to relieve the weakened and dilated capillaries. Fortunately for the patients whose cases I have detailed, and for many more, I was not reduced to the sad necessity of taking blood to diminish the excitement of the heart and larger arteries, because I conceived such excitement did not exist. Their pathological condition I considered as arising from weakness, a state similar to that which the smaller arteries of an inflamed part acquire. I had not recourse to local blood-letting to relieve the weakened and dilated capillaries, for, as I conceived they became dilated in consequence of weakness, I considered it better to enable them to relieve themselves.

Whether my views be correct or not, time, the observations of others, and perhaps experiment, may prove; but I offer in support of their soundness, the *modus operandi* of the remedy made use of, until it be otherwise explained than after the manner in which I attempt.

That the liniment I employ does not act on the vascular system through the absorbents, that is, by any portion of the components of it being absorbed, I think will appear evident from the fact, that in some cases the good effects follow the application almost immediately. From the fact likewise, that, although I have applied it in very large quantities at once to very young children, I have never seen any unpleasant effect produced; yet it is recorded that fatal results have followed the application of a single grain of opium to blistered surfaces in young children. Indeed, I do not believe that absorption is a function natural to the skin in the human body; it rarely, if ever, takes place under ordinary circumstances, the cuticle being entire; under peculiar circumstances, we have proof that it does take place.

Captain Bligh and his unfortunate companions, when cast adrift by the mutineers of the Bounty, found some relief from an almost insupportable thirst, by dipping their shirts in the salt water, and then wrapping them round their shoulders. This, however, soon failed in its effect; yet as it did for a time, in some degree, assuage thirst, physiologists claimed the fact as a proof of the absorbing power of the skin. I remember with how much delight a distinguished professor used to dwell on this unquestionable proof; and at a later period with what pleasure I confirmed its truth, as I thought, in my own person, by experiment, when often, towards the end of a long march on a sultry day, I have relieved my thirst by merely washing the face and hands in a cool spring, and refraining from tasting that the absorbents might have all the credit. Most certainly I would have been greatly astonished, had I then been told that the absorbents had nothing to do with the effect produced.

In continued fever I have in many cases observed that when opium, rubbed up with lard, was applied to the surface of the body, the thirst became less and less urgent, and the tongue, which before was parched and incrusted, became moist and clean. The cold affusion or sponging with vinegar and water produces a like effect, and in this case we content ourselves with the explanation which presumed absorption affords; but where opium is employed we must attribute the effect to some other cause. I have relieved thirst in my own person, and in that of others, by the application of cold dry substances to the surface, and therefore I am inclined to believe that it was the action of cold on the surface of the body which produced the relief in Bligh's case. The skins of his crew were scorched by the rays of a tropical sun, and therefore the water which, under other circumstances, might have imparted warmth, must have, by its comparative coolness, allayed the excitement of the sentient nerves of the skin, and thus have determined a force of nervous influence to internal surfaces, sufficient to restore, for a time, their secretions.

Since writing the above, I find Captain Bligh's evidence appealed to as an argument in favour of absorption by the skin, in the Medico-Chirurgical Review, April 1833. There the reviewer of Dr. Wood's Essay on the Structure and Functions of the Skin, says,—"Nor are we to reject the evidence of Captain Bligh and others, on the astonishing and most pleasing effects of wetting the surface to allay the thirst and burning, when their fresh water had failed them."

If a person become thirsty from fatigue and heat of the weather, let him expose the neck and breast, and lie down on his back; then let an attendant pour a bag of small shot, taken from a cool place, upon the exposed surface, and his thirst will be relieved. The pleasing effect in this instance cannot be attributed to the absorption of moisture.

The phenomena resulting from the application of opium to the surface of the body resemble so closely those arising from cold, that it is impossible to conceive a difference in the mode of their operation. When applied extensively to the surface of the healthy body, opium diminishes the frequency of the pulse, accelerates the peristaltic motion of the intestines, increases the internal secretions, creates hunger, produces the sensation of cold, and even shivering. Inactivity or torpor of the sentient nerves of the skin is the immediate effect of the opium, and thus one great drain on the general expenditure of nervous influence is obstructed, consequently the stream of nervous influence is directed in greater force through other nerves, by which their functional activity is increased. If the application of the opium be extensive or long continued, the contractile power of the arterial system becomes so much increased that a tendency to continued contraction produces venous plethora, and sleep is the result.

Cold, in producing sleep, acts in the same manner, and experience has taught us that the only method of preventing that tendency to continued contraction ending in death, is to divert from the heart and arteries the extra force of nervous influence derived from the sentient system of nerves into some other channel, namely, to the motor nerves, by muscular exertion.

In diseases where the heart and arteries labour under the deprivation of nervous influence, owing to excitement of the sentient nerves, the remedy is to allay that excitement, and you restore vigour to the arterial system. That vascular commotion, mistakenly called vascular excitement, originates in and is maintained by deficient energy on the part of the arteries. In every case which I have detailed, the action of the opium in restoring the vigour required is strikingly manifest. In Case III., that of Race, the pulse, which was upwards of a hundred and sixty, was almost immediately reduced to a hundred and twenty. I suspect it to have been lower, and that it was again on the increase at our second visit; at any rate it was reduced considerably below the natural standard in less than twelve hours. This was a case which we considered hopeless, and, therefore, that the liniment might have a fair trial, we refrained from administering any thing internally.

Without condemning the practice of any one, I may here ask what would have been the probable result had we leeched and blistered in this case? By relieving, as it is called, the weakened and dilated arteries, without, at the same time, restoring their contractility, we would have taken from them the stimulus of distention, which prevented them being totally deprived of the power of contracting, and then they would have become lifeless tubes, through which the serum of the blood would have passed to suffocate our patient. We would, in this case, have proved, by experiment, the truth of Dr. Hall's conclusion, that another effect of exhaustion (from loss of blood) is effusion into the air-cells and bronchi giving origin to a peculiar rattle heard with the stethoscope, and generally of fatal omen.

It will be observed that in all my cases, the change from disease to health is so sudden, that there is no, what is called, convalescent period. By restoring the equilibrium in the distribution of nervous influence, we restore the strength of the patient, so that in every case, the physician is not so helpless, as the author of a late work on fever would deem him. He says the physician can easily weaken, but he cannot easily strengthen; he can depress to any extent he desires, but he cannot communicate power as he wishes; and I agree with him that, most certainly, we cannot, if we ourselves, by depleting measures, create the weakness.

Bronchitis, says another author, is almost invariably followed by great debility, both of the general and the pulmonic system. At one time I could have acquiesced in this opinion, for I found it to be correct so long as I conformed to the established rules of practice; but, from the time that I presumed to think for myself and to act accordingly, I have observed neither general nor local debility, and therefore the debility formerly witnessed I must have caused by the means resorted to in effecting the cure. I expect to see the day when blood-letting as a remedy for inflammatory attacks, in children at least, will be as much decried as it is at present lauded. Not that I flatter myself my opinions will go far to effect the change, but I think there is a tendency to doubt the infallibility of depleting measures, rapidly diffusing itself throughout the profession. Dr. Stokes, of Dublin, is now giving opium in large doses in cases in which he thinks that, from debility or other causes, blood-letting is inadmissible, and that too with the greatest success.

He thus proves that in many cases venesection is, at least, not necessary, and I doubt not that in his future reports we shall read of similar success in robust habits. Were it not for that *dreaded increased vascular action,* which, we are told, *must* be subdued before we ought to have recourse to calomel and opium, these medicines, instead of being employed as auxiliaries, would ere this have been the prime agents in the cure.

In inflammation of the brain or its membranes, it is thought good to apply ice to the scalp, and the benefit resulting is doubtless to be attributed to the secondary effects of cold. "Cold acts sedatively: by producing torpor of the nerves to which it is applied, it renders them insensible to the impressions which naturally elicit nervous influence to their extremities; this is its primary action. Cold acts excitingly: by producing torpor of the nerves to which it is applied, it renders them useless as instruments of transmission of nervous influence; therefore that force of influence which otherwise would have been transmitted towards their extremities, is directed through other nerves, increasing their power of action; this is its secondary effect, or the effect of its primary action."

The benefit, then, from cold applied to the scalp in inflammation of the brain, arises from its secondary effect; the capillaries, indeed all the arteries of the brain and those leading to it, gain an increase in their power of action, and are thus enabled, by forcible contraction, to unload themselves and oppose re-distention. During the application of cold to the scalp the carotids pulsate less frequently and less forcibly, evidently demonstrating that they contract more forcibly on their contents. Yet it is conceived that the perceptible change in the action of these vessels is owing to the sedative operation of the cold extended to them, by sympathy, from the vessels of the integuments. Whether this be the opinion generally entertained as to the operation of cold applied as above, I know not, but it is that of the author of the article "cold" in the Cyclopædia of Practical Medicine, which is intended to be a standard work, and which in all likelihood will be widely circulated. "Cold," says Dr. Whiting, "applied to the scalp will be found to act as a sedative, not only upon the vessels of the integuments, but (by a sympathy well known to exist between the larger and the smaller vessels) upon the carotids also. It must be clear, therefore, even from theory, that cold applied to the integuments of the head will be of service in inflammatory diseases of the brain." Did cold act as a sedative upon the vessels of the integuments, their muscular coats would become relaxed, and distention would follow, giving to the surface an appearance such as is produced by a rubefacient. Cold, however, in this case does not act as a sedative upon the vessels, but as a sedative upon the sentient nerves, throwing them into a negative state; the nerves, through which the contractility of the neighbouring arteries is furnished or excited, acquiring at the same time a positive state: hence, instead of the contractility of these vessels being impaired, as would be the case were a sedative directly applied to them, or extended to them by sympathy, it is increased, whereby their power of resistance is augmented.

Opium and cold have a like *modus operandi* when employed externally; therefore, when I apply opium to the integuments covering the chest for the purpose of curing internal inflammations, I but use a remedy which acts on the same principle as the remedy employed by others to cure internal inflammations elsewhere.

Amongst the remote causes of inflammations of the viscera of the chest, we find cold, and therefore I presume it is that its external use in those diseases has not gained ground. In inflammation of the lungs, I certainly would not advise a sudden and extensive application of cold water to the surface of the body, for, by causing the weakened and distended vessels of the part inflamed to contract suddenly and forcibly on their contents, we might produce rupture; yet this fear may be more fanciful than real, for in inflammation, when syncope is produced by venesection, the arteries then contract more suddenly and forcibly than we can make them do by any other means, and syncope often occurs before a couple of ounces can be drawn, nay, sometimes before the vessel can be cut. Dr. Whiting says, "it seems to remain doubtful whether or not cold may with advantage, or even with safety, be applied to the chest or abdomen when there exists inflammation of the membranes or viscera within those cavities. This practice has however, of late, been recommended by some physicians whose opinions ought to have weight, and especially so, as the recommendation seems to be grounded on the result of experience."

My own experience does not as yet justify me in saying that I can rely altogether on the external use of opium for the cure of inflammations of the chest in adults, for in one case of pneumonia of a threatening aspect, I was forced to use the lancet, principally, however, because the disease was of some standing, and I thought it was one which promised to end fatally, and in that case, had I not bled, I would have been blamed as ignorant or rash. As it was, the patient lost but sixteen ounces, and felt no relief until the opiate liniment was very liberally applied. During three years the above is the only instance in which I have bled, and if many of the cases which have come under my care during that period, and which were treated with opium externally and internally, were trifling in degree as to severity, yet were they such cases as formerly I would have treated by blood-letting, indeed, I would have held myself culpable had I neglected it.

As it was my intention to confine myself as much as possible to the treatment of the two diseases named in the title, I shall only here further allude to another observation of Dr. Whiting's;—he says, "in some cases of *phthisis pulmonalis*, where the pleura has become inflamed, an evaporating lotion applied over the part in pain has often appeared to be of considerable service." In such cases I would advise an opiate liniment; it acts upon the same principle; a minute will suffice for its application, and its effects will be found to be more permanent.

General blood-letting, external cold, and opium externally applied, all act on the same principle in effecting the cure of inflammation. They operate by causing a diversion of nervous influence in favour of the arterial system, by which the contractile force of the heart, larger arteries, and capillaries, is augmented, so that they are enabled to overcome the distention which their weakened condition permitted.

There is another remedy supposed by some to possess a specific influence in checking the progress of inflammation, and if we examine the mode of its operation we shall find that it likewise acts on the above principle: I mean tartar emetic. As with blood-letting the benefit derived is not from the quantity abstracted, but from a certain state of the general system produced by an impaired operative condition of the brain, so is it with tartar emetic; the benefit which flows from its administration arises from a certain state of the general system which it produces, the distinguishing feature of which state is *nausea.* It is a state similar to that of approaching syncope in which the functions of the brain are all but abolished, and this is the condition so favourable to the solution of inflammation, whether it be brought about by blood-letting, tartar emetic, ipecacuan, or any thing else. Tartar emetic is to be preferred, because we can regulate its dose so as to prolong nausea. Whilst the operative state of the brain is impaired, of which nausea is a symptom, there is a greater than natural determination of nervous influence to the sympathetic system of nerves, consequently the heart and arteries gain in contractile power, and thus unload themselves by vigorous contractions. From this great determination of nervous influence to the sympathetic system during nausea, some of the secretions are also greatly increased; "*vomitum præcedere solet ingens humorum oris fluxus.*" Not that I believe that increase in some of the products of secretions tends much to forward the cure, but I mention the circumstance merely to shew more clearly, that, if there be diminution of some of the powers of the nervous system during nausea, there is augmentation of others.

Blood-letting and tartar emetic in the cure of inflammation operate by producing a change in the distribution of nervous influence, but, before this change can be effected, a general disturbance of the different functions of the body must first take place, therefore they act indirectly, and are not to be depended on. Indeed, no one ever thinks of trusting altogether either to the one or to the other. Opium, as I employ it, changes the distribution of nervous influence at once, without visible disturbance of any one function. It allays the excitement of the sentient system of nerves, to support which excitement the arterial system had been robbed of its contractility: this excitement being allayed, the heart and arteries recover their power, and healthy action is restored.

In the London Medical and Physical Journal for July 1833, I find the following. It is taken from Mr. Adams' Pathological Observations; Glasgow Medical Journal. "Dr. John D. Godman * narrates, that his children have been very liable to croup, and that he was informed by Dr. F. Vanderburgh of a very simple and efficacious method of arresting, at once, all the symptoms of this frequently destructive disease. Whenever children are threatened with an attack of cynanche trachealis, he directs a plaster, covered with dry Scotch snuff, to be applied directly across the top of the thorax, and retained

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* American Journal of the Med. Sciences, No. IV. August, 1828.

there till all the symptoms disappear. Dr. V. stated that he had found this remedy always effectual when applied in the first or second stages of the malady. Dr. Godman has tried this plaster repeatedly, (made by greasing a piece of linen and covering it well with Scotch snuff), and has found it abundantly efficacious. His family have been saved a great degree of anxiety since, being quite prepared for the attacks, which are frequent in cold and wet seasons. Whenever a child is hoarse, or coughs with the ringing of croup, the plaster is applied, and instead of watching all night, the family go to rest with a feeling of entire security." This remedy is simple enough, and I have no doubt of its efficacy, notwithstanding, I dare say, many have laughed at the idea that a disease, which so often defies the lancet, should be thought capable of yielding to a little snuff. Since I first saw the above quotation, I have used a plaster composed of soap and extract of belladona for cough with the ringing of croup, and in a few minutes after its application the sound of the cough became changed.

In iritis no one would neglect the external application of belladona, for in this disease we immediately see the effect of its action; shortly after its application, the weakened and distended vessels composing the iris are seen to contract. This effect does not arise from any mode of action peculiar to belladona, or from any specific influence it possesses over the iris. It acts upon the principle I have attempted to explain, and, with regard to the iris, perhaps more energetically than opium, or henbane, or the other narcotic vegetables. On the same principle by which it causes contraction of the iris, does it cause contraction of the weakened and distended vessels of the inflamed mucous coat of the air passages, when applied to the throat and chest.

Blisters, although recommended in croup and bronchitis, I must of necessity condemn; for if the liniment I employ acts in the mode in which I presume it does, then they, by exciting the sentient nerves of the surface, must aggravate the disease. When I followed the routine practice, I never employed them from choice, for, at all times, authors, even whilst they advised them, spoke doubtingly of their efficacy.

Those who believe that inflammation consists in an excited state of the vessels of the part, may have a plausible pretext for the practice, and they can only be undeceived as to their utility by witnessing the mischief which may from time to time follow their application. Those, however, who believe in a weakened state of the vessels of an inflamed part, still, unaccountably, urge the old practice of blisters; but as they dare not justify the practice on the score of counter irritation, lest by that they acknowledge a doctrine they deny, they tell us that blisters act beneficially by directing the fluids to the surface, by which they relieve the congestion of the debilitated capillaries.

Since my theoretical views lead me to condemn blisters, and as I have no knowledge of their good or evil effects from experience, I shall here make use of a few extracts, which I think prove my opinion to be correct. Under the article *Counter-irritants*,—Cyclopædia of Practical Medicine, I find the following:—"Experience continually proves that under certain conditions of the system, a blister or other counter-irritant aggravates the inflammation which it was applied to remove." "In common with other remedies of this class (counter-irritants) they (blisters) are unfit for the first stages of extensive acute inflammation." "But equally as in the cure of acute inflammations, during the stage of synocha or morbid excitement, they (blisters) must be excluded as injurious."

And under the article *Bronchitis:*—"There is an objection which obtains so against blisters, as to exclude their use in many cases of the early stage of acute bronchitis; namely, that, before they rise, they always produce an irritating effect on the whole system, which is very apt to aggravate for the time the inflammation for which they were applied."

Dr. Copland says, "great mischief has arisen from ordering blisters and mustard poultices too early in inflammations, but more particularly in encephalitis, when instead of deriving the circulation from the inflamed part, they excite the nervous and vascular systems generally, and thus react upon the disease."

Inflammation consists in a morbid excitement of the sentient nerves of the part, and when sympathetic fever is formed, there is excitement of the whole system of sentient nerves; if the object be to increase this excitement, then apply blisters, mustard poultices, &c.; but if the object be to allay this excitement, and thereby restore to the arterial system its power of resistance, apply sedatives, such as cold, opium, belladona, &c.

Before concluding, it will be necessary to say a few words about the mode of applying the liniment, for in some cases the expected benefit did not follow its application, as, for instance, in Case VI. I found that the quantity ordered was put into a tea-cup, and then by means of one finger applied by degrees to the chest. The greater the extent of surface to which it is applied the better. When I possibly can, I see the first application; I order the child to be stripped, and, from the vial, pour on the breast about a drachm at a time. It should, however, be in such quantity as to force the nurse, to prevent it running down the sides, to apply it rapidly and extensively. There is nothing to be feared from quantity if the skin be very hot: the child, Case VI., was but two months old, and there was at least half an ounce employed at once. It is not necessary to rub it *in;* as soon as it has dried sufficiently, so as not to be absorbed by the linen, the child should be placed in bed, and, in most cases, if the liniment be freely used, a second application may not be necessary.

I have not as yet met with a case, either of croup or bronchitis, which has not yielded to the plan I propose, with the exception of one, and that case I did not see until the poor child was exhausted by the disease and depleting measures. I have, therefore, reason to feel the greatest security in the plan, and I trust that if what I have written be seen by any of the medical gentlemen who have the charge of institutions for the reception of children, they will give it a fair and full trial. As visionary, they may regard my views of the pathology of the diseases and of the *modus operandi* of the remedy; but I am confident they will have it in their power to acknowledge a happy result from the practice.

PHYSIOLOGY

OF THE

THYMUS GLAND.

Convinced that an increased activity in the function of any organ of the body cannot obtain unless a greater than usual force of nervous influence be determined to that organ, and that this increased force of nervous influence must be furnished at the expense of other organs, it naturally enough occurred to me to inquire from whence is derived the great supply of nervous influence so suddenly demanded by the chemical and the mechanical respiratory apparatus at the moment of birth: a supply, too, never to be restored to the organs furnishing it. The Thymus Gland, as the organ at whose expense this supply is yielded, immediately presented itself to my mind; and the gradual wasting of the gland after birth, it having performed its function, I considered testimony sufficient to support such inference. The thymus, I therefore concluded, expended in the fœtus that force of nervous influence which was destined for a particular purpose after birth; and I presumed that by this contrivance the safety of the fœtus in utero was insured, and also the safety of the child on its first inspiration. The thymus, however, expending so much nervous influence in the fœtus, must be endowed, I thought, with a function yielding a product; consequently I referred to what was known of the anatomy of the gland, and found that vessels resembling lymphatics passed from it to the thoracic duct, and that the gland and these vessels often contained a milky fluid so like to chyle, that Dr. Tyson supposed the use of the thymus to be for a *diverticulum* to the chyle in the thoracic duct in the fœtus.

This fluid must be useful to the fœtus; and as it is poured into the thoracic duct, from thence to be circulated with the blood, its use, I concluded, could only be that of enriching the circulation to further nutrition, and I did not hesitate to publish, as my opinion, that the thymus acted in the capacity of a stomach to the fœtus.

This view of one of the ends gained by the design of the thymus, was, to me, confirmed by the fact that those animals in which it is proportionally large are, after a short gestation, brought forth as mature as other animals in which it is proportionally small, after a lengthened gestation.

These being my views regarding the physiology of the thymus, it gave me great pleasure to find, on the publication of Sir Astley Cooper's work on the Anatomy of the Gland, that the distinguished author thought it "probable that the gland is designed to prepare a fluid well fitted for the fœtal growth and nourishment from the blood of the mother before the birth of the fœtus, and consequently before chyle is formed from food, and this process continues for a short time after birth, the quantity of fluid secreted from the thymus gradually declining as that of chylification becomes perfectly established."

This opinion as to the use of the thymus, coming from the quarter it did, demanded attention, and it was lauded by some of the periodicals as ingenious. But as I had anticipated Sir Astley in giving the same office to the fluid secreted by the gland, I thought there could be no harm in drawing the attention of the Editor of the Edinburgh Medical and Surgical Journal to a passage or two of mine, and he was polite enough to notice them as follows:—

"*On the Uses of the Thymus Gland.* By Dr. WILLIAM FORRESTER BOW, *Alnwick.*—(*Notions of Fever and of Nervous Action*, 1829.)—Since the foregoing account of the work of Sir Astley Cooper on the Thymus Gland was printed, our attention has been directed by Dr. Bow to a passage in his work above-mentioned, in which he conceives he has anticipated Sir Astley Cooper in the uses which that able anatomist and surgeon assigns to the *thymus gland.* We freely confess, that when we perused the work of Sir Astley, we were most impressed with the value of the anatomical facts, which are precise and specific; and to the conjecture about the uses of the organ, though ingenious, we felt that what was given as a mere query, was not intended to be made a very serious matter; and from habitual caution in all opinions and speculations on uses of organs, we attached no importance to the conjecture, in comparison with the attention which we gave to the matters of fact, and expressed no opinion as to its merit.

“ Under these circumstances, we should scarcely have been induced to attach more importance to the mere opinions of Dr. Bow, than to those of Sir Astley Cooper. Desirous, however, at all times to give publicity to all ingenious conjectures on obscure and little understood subjects, and also to allow Dr. Bow whatever merit his conjectures may possess, we willingly give the passages to which he refers in his work, which appeared in the year 1829.”

‘ To the THYMUS and the SPLEEN the physiologist has not as yet assigned any definite function. I therefore trust I may not be deemed guilty of arrogancy, if I claim for these glands the office of expending, in the fœtus, the nervous influence destined to flow to the respiratory and digestive apparatuses after birth. In the *thymus* gland, the blood is operated upon by nervous influence supplied by branches from the *par vagum;* it is, as it were, reconverted into chyle, which is conveyed to the thoracic duct to enrich the circulation. The thymus thus acts in the capacity of a stomach to the fœtus. As soon, however, as respiration commences, a new order of things obtains: the lungs and their appendages are furnished with nervous influence at the expense of the thymus gland, which now withers, and gradually diminishes in size.’

‘ By the contrivance of the thymus gland, not only is danger averted, but a power which otherwise would have been perilous to the fœtus is directed to assist in its nutrition.’

"After giving the opinions of Dr. Bow in his own words, we wish to leave our readers to form their own judgment of their merits. In one respect these opinions proceed further than the conjecture of Sir Astley, and therefore do not exactly coincide; and that is in representing the *thymus* and the process supposed to be carried on in it, as a sort of vent or safety valve to dispose of the superfluous nervous energy, which is conceived to exist, and which is after birth required for the function of the lungs and the alimentary tube.

" On the other opinion, regarding the use to which it is believed the thymic fluid is applied, Dr. Bow must not be displeased when we say, that in his work it appears in the form of a conjecture only, unsupported by any fact or satisfactory argument; whereas, though the opinion of Sir Astley is also conjectural, and merely in the shape of a query, it was suggested by the anatomical fact originally observed by Wharton and Drelincourt, of the large lymphatic vessels terminating in the subclavian vein, and by the milky fluid of the thymic cells being proved by demonstration to be capable of flowing in that direction, and in that only towards the subclavian vein. We are satisfied that Sir Astley Cooper will be pleased to find that Dr. Bow has, however circuitously, arrived at the same end."—*Edinburgh Med. and Surg. Journal*, No. CXIII. Oct. 1832.

Every one must admire the zeal and dexterity with which Sir Astley Cooper has investigated the anatomy of the gland; yet it is one of those parts, the anatomical knowledge of which can lead to nothing beneficial either to the physician or the surgeon. The pains taken to acquire that knowledge, however, may be amply requited if it be pointed towards the gland's physiology. But its physiology, it would appear, is a matter of very little moment, and, because it comes in the form of conjecture only, it is not worthy of comment, even though it is Sir Astley Cooper who conjectures. And, be it remarked, Sir Astley's sole object, in ascertaining the structure that produced the secretion, in examining the cavities containing it, in tracing and injecting the vessels by which it is carried away, and in learning the nature of the fluid itself, was "to form a probable conjecture of the use of the Gland in the fœtal and infantile life."

I did not blame the editor for not attaching importance to my opinions. I merely pointed out to him the coincidence of opinion regarding the function of the gland. And surely it is excusable to claim a share of the merit bestowed on the "ingenious" conjecture of Sir Astley, when the humble individual who does so, shews, at the same time, that he was the first to promulgate the conjecture. If the view now taken of the use of the gland prove erroneous, it will go with former and now forgotten speculations; but if it gain support or be confirmed by future observation, my claim will detract nothing from the fair, the great, and the deserved fame of Sir Astley Cooper.

I am not displeased with the editor for saying that my opinion regarding the use of the thymic liquor is conjecture only, unsupported by any fact or satisfactory argument. It was a speculation, and advanced as such, grounded, however, upon anatomical facts, the possession of which might have been inferred, when I said that the blood "is, as it were, reconverted into chyle, which is conveyed to the thoracic duct to enrich the circulation." According to the editor, Sir Astley Cooper had no surer foundation for his conjecture than I had; we arrive at the same end, and I thank the editor for acknowledging this fact; but to mark how grudgingly the acknowledgment is conceded, my route must needs be circuitous.

From the view I have been accustomed to take of the distribution of nervous influence and the laws regulating that distribution, I saw, or believed I saw, the necessity for the existence of some contrivance to hold in play a power destined for a future purpose, and in the thymus I found an organ beautifully adapted to this end. An organ which, at a moment's notice, can with safety to the animal yield its vitality, and which, having performed its office, is gradually removed. Such I believe to be the end for which the thymus is moulded. Without this or some other similar contrivance, the first inspiration would undoubtedly prove fatal, for from whence could be derived the nervous influence necessary to its accomplishment?

I suspect that all those gastro-intestinal inflammations which occur in new-born infants may be traced to some imperfection in the thymus gland. It may be so imperfect as to be unable to furnish the whole force of nervous influence necessary to respiration, in which case the digestive apparatus may be deprived of its due share to supply the deficiency, and thus become morbidly affected. Or it may be so imperfect as to be able to furnish little or none, in which case the heart itself may be deprived of its nervous influence on the first attempt at respiration; and cases are not wanting to prove that the first inspiration is oftentimes the last.

With regard to the thymic liquor, I could come to no other conclusion, circuitously or not, than that its use is to enrich the circulation.

The design of the gland is to expend a power, the principal use of which is prospective; but as in the laboratory of creative wisdom every thing is turned to good account, so, resulting from the expenditure of this power, we find a product, and this product not only resembles chyle, but is conveyed, as chyle is, to mingle with the circulation. It is impossible to conceive that this liquor is intended to deteriorate the blood, and it is as impossible to conceive it to be of no use.

I confess I have no satisfactory argument by which to prove that the above is the use of the thymic fluid, but the conjecture is supported by some facts which do give it an air of probability. Supposing its use is to further nutrition, we are prepared to expect that those animals in which the gland is comparatively large, will be brought forth after a short gestation as mature as others in which it is comparatively smaller, after a lengthened gestation: this we find to be the case. In the calf and the lamb it is comparatively very large, and they are brought forth, after a fœtal existence, the one of nine months, the other of five, as mature as the foal of the mare and of the ass, although the one is carried eleven, and the other twelve months.

"In the Pig, the Gland is formed of two thoracic portions of an isthmus and two cornea; its lobes are of considerable size, and minutely subdivided into smaller lobes, and the gland is very large in proportion to the weight of the animal.

"It contains secretory cavities so full of fluid, that it is very difficult to inject them." *

Of all the mammalia there is none brought forth more mature, or fitter to provide for itself, than the pig, yet the term of its fœtal life is only sixteen weeks, whilst in man, although the fœtus has an existence of nine months, it is born the most helpless of all. In the human fœtus, at birth, the gland is of small size, not exceeding half an ounce in weight, but in the pig it is "very large in proportion to the weight of the animal."

Buffon says, that with "all animals which produce in great numbers, the young ones, at the time of their birth, are not so perfect as those of animals which only produce one or two." The learned naturalist, however, it appears to me, was too hasty in this conclusion, for in the history of many animals he produces evidence to the contrary. For example, he could not have been ignorant of the mature state of the pig at birth; yet, as he says, the sow not unfrequently produces eighteen or twenty young. The female Guinea pig, he observes, never goes with young above three weeks, and she has been known to

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* The Anatomy of the Thymus Gland, by Sir Astley Cooper, Bart., F. R. S.

bring forth when only two months old. In general, when only five or six weeks old, they begin to copulate. The first litter does not amount to more than four or five, the second to five or six, and the rest to seven or eight, or even to ten or twelve. The dog, when he is born, Buffon quaintly observes, is not entirely finished; and, apparently to apologize for his unfinished state, draws the sweeping conclusion which I have quoted above. The Guinea pig, however, may be one of ten or twelve at a birth; its mother suckles it twelve or fifteen days only; it becomes salacious when five or six weeks old, and, if a female, it may be a mother, after three weeks gestation, at the age of two months. The membrane uniting the eyelids of the puppy is not torn until the tenth or twelfth day, about the time that the Guinea pig is left to shift for itself. If the history of the Guinea pig, as given by Buffon, be correct, it must be born even more mature than the pig, and as its fœtal existence is of so short duration, its thymus gland must, I think, be very large in proportion to the weight of the animal.

Sir Astley Cooper thinks it cannot be doubted but that the function which the thymus performs is highly essential to the existence and growth of the fœtus and *infant*. If, however, the real design of the gland be to keep in play the nervous influence destined to be afterwards otherwise employed, the product of the gland after birth must, in the human subject at least, be very trifling, and therefore its function can be not only not essential, but of very little use to the infant. I cannot guess the reason for supposing its function to be essential to the infant. "At eight months" (fœtal life), Sir Astley says, "it (the gland) is large, but at the ninth month has undergone a sudden change, becomes of great size, and is said to weigh half an ounce, from which circumstance, however, on account of the cavities which it contains and the varieties to which it is subject, no judgment of its bulk can be formed. It increases after birth, and continues large to the first year, when it slowly disappears to the time of puberty; and in after age it ceases to have cavities, and becomes a body of very small dimensions."

The increase of the gland after birth, and its continuing large until the first year, are circumstances which might lead us to think its function essential to the infant. But Sir Astley, at page 38, says, "The office which the Thymus is designed to perform is evidently connected with the fœtal stages of existence, as it gradually lessens *soon* after the child is born, and even when the Gland remains of considerable bulk, its secretory cavities are much diminished."

Again, at page 43, Sir Astley says, "It appears to be an error to suppose that this Gland continues for some time after birth to perform the same office as that which it supported in the fœtal state.

"I injected the Gland in a child of one month, and I found that the lobes had become quite thinned by absorption, although the reservoirs in part remained, but even one of these was partially obliterated. In a child of four months the reservoir was very small, broken into several portions, and the weight of the Gland which should have been in the fœtus of nine months 240 grains, weighed only 45 grains, or about five times less than in the fœtal state."

I suspect that in the human subject there is not one drop of thymic liquor secreted after respiration is established, and that that which is found in the reservoirs and absorbents of the gland after that period is that which was there at the moment of birth; the absorbents, being part of the gland, lose their vitality likewise, and therefore cannot forward their contents to the veins.

In the calf, however, the function of the thymus gland may not altogether cease at birth, for in ruminant animals there is still a necessity for a contrivance to hold in play a force of nervous influence, the use of which, even at birth, is prospective. While the ruminant animal is supported by its mother's milk or other nutritive liquors, the first and second stomachs remain quiescent; the milk passing directly through the third into the fourth stomach. When rumination commences, which, according to circumstances, may not happen, in the calf, before the end of four months, it is evident that a new secretion will then be required for the maceration of the aliment in the first and second stomachs, and as at this period also there is a wonderful increase in the production of saliva, the power to produce and to augment these secretions must be granted at the expense of some organ which can bear the loss without detriment to the animal. The thymus is doubtless that organ: at four months, in the child, we find it more than five times less in weight, than at birth, with the reservoir very small and broken into several portions; whereas, in the calf of four months, the size of the gland is larger than at the birth of the animal, with cavities diminished by two-thirds only, instead of being very small as in the child.

To me it appears that the thymus, in the calf and other ruminant animals, holds in play a greater force of nervous influence than is necessary for the respiratory apparatus, and, therefore, although its secretion becomes greatly lessened after birth, the power effecting it being in a great measure withdrawn, still the function is exercised until rumination is established. To effect the secretions necessary to rumination, the thymus at length yields the remainder of its nervous influence to a plexus of nerves (found in ruminant animals only) situated upon the œsophagus, and first described by the late Mr. John Shaw.

The thymus is not the only contrivance intended to expend in the fœtus the nervous influence required by organs which do not come into play till birth: for instance, the stomach has its spleen, the kidney its capsule, &c. The spleen, however, does not disappear by degrees, like the thymus, because the process of digestion, unlike that of respiration, is not in constant operation. The spleen, in the fœtus, diverts from the stomach the nervous influence which after birth effects digestion; and, in the born, it diverts the same power from the stomach when digestion in that organ is finished.

Were it not for the spleen, I thought there could be no cessation to the production of the gastric juice, and that the animal would, in consequence, be incommoded by a constant craving for food. The correctness of this opinion, it struck me, might be decided by a reference to the observations of those who had removed the spleen from the living animal. Accordingly I found that Malphighi and Dupuytren remarked the voracity exhibited by animals so treated, and the subjects of the later experiments of Mr. Dobson, I find, all ate voraciously. Had I instituted experiments similar to those of Mr. Dobson, predetermined to see nothing but that which might support my own views, I could not have been more successful than in my gleanings from his observations.

If the nervous influence necessary to digestion in the stomach be derived from the spleen, it must necessarily follow that the chemical nerves of the spleen, during digestion, will be thrown into a negative state, consequently other nerves of the same organ, those which effect its vascular contractility, will acquire an opposite or positive state, by which, the blood-vessels being forced to powerful contraction, the size of the spleen will be diminished. Digestion being ended, the chemical nerves of the spleen will again acquire the positive state, and, as a necessary consequence, the other nerves of the organ must assume the negative; therefore the contractility of its vascular structure will again be diminished, and thus opposition to the influx of blood being removed, the spleen will increase in size. Mr. Dobson confirms the testimony of former observers as to the varying size of the spleen; it is of large size when the stomach is empty, for a certain time at least, and small when the stomach is full.

If the nervous influence necessary to digestion be derived from the spleen and restored to it when digestion in the stomach is completed, what becomes of this nervous influence in those cases in which the spleen has been removed? For a solution of this problem I shall take the liberty of referring to the relation of what took place in the dogs on which Mr. Dobson experimented.

"Exp. I.—The spleen of a dog being removed; the animal apparently suffered little pain from the operation. On the following day I gave it a quantity of food; it ate voraciously; for three hours after no perceptible alteration was produced; but in *four hours* after, indications of uneasiness were shewn; the animal became restless, and lastly sunk into a torpid state; it was often moaning, the pupils were dilated,—the heart labouring; there was frequent micturition; the respiration was exceedingly laborious, and, in short, there was every mark of plethora, or over-fulness of the vascular system. In the course of two hours from this period, the animal began to recover; and in about three hours these symptoms had subsided; considerable languor remained. The animal took a large meal twice or thrice in twenty-four hours, and after each, precisely similar effects were presented. The animal became more feeble daily,—in a month after the operation, it died."

"Exp. II.—I next removed the spleen from another dog, but instead of giving full meals, as in the last experiment, I gave a small quantity of food every hour, or every two hours. The animal ate voraciously; no unpleasant symptoms occurred,—this plan was pursued for three weeks, when the animal to all appearance was quite well; in fact, it became fat; the ligature upon the splenic artery had come away, and the wound in the abdomen healed. I then commenced giving full meals twice in twenty-four hours, the same train of symptoms followed each meal, and at the same period, as in the last experiment, though perhaps not so urgent: the animal died in a month from the commencement of this plan of feeding."

"In both dogs I observed that the intestinal evacuations were of a *lighter* colour than natural. On examining the body of each after death, a small quantity of limpid serum was contained in the bag of the Tunica Arachnoides, and more than a natural quantity in the lateral ventricles; the veins of the brain were in a highly congested state; the abdominal viscera presented no unnatural appearances, but the portal system of veins was much gorged with blood."

Mr. Dobson believes that there is a greater quantity of blood in the system at five hours after a meal than at any other period, and supposes that the blood-vessels are not capable of containing this increase with impunity; and he infers that the spleen serves as a reservoir to hold this surplus, because, at the time the chylifactive process is at an end, the spleen is found distended with blood. The spleen being removed, there is no longer a reservoir, and the distress of the animal four hours after a meal is imputed to the blood-vessels being unable with impunity to contain the surplus blood; indeed, the uneasiness of the animal, according to Mr. Dobson, is characterized by every mark of plethora or over-fulness of the vascular system.

I do not refer to Mr. Dobson's experiments for the purpose either of criticising or impugning the inferences he has thought proper to draw from the facts he has noted; yet, as I may infer otherwise from the same facts, I trust, if I attempt to weaken his position, I may be excused, for I shall do so merely to strengthen my own. If, during digestion in the stomach, all the secretions were at a stand, I could then conceive how the blood-vessels might, four or five hours after a meal, be forced to receive more than would be consistent with healthy action. This, however, is not the case, and, laying aside every other secretion, it is admitted by all that a product in the liquid form is necessary to reduce the aliment in the stomach to a chymous pulp, and that this liquid, whatever change it may have undergone from the action of the nerves of the stomach, is derived from, or separated from, the blood. This pulp, after it passes through the pylorus, is rendered more liquid by the admixture of other products, also derived from the blood; so that the chyle, before it become sufficiently fluid to enter the lacteals, may have had spent on its production, with that which remains with the refuse matter, a volume of fluid equal to its own volume, or greater if the aliment be very hard and dry, as is sometimes the case with the food of dogs. Besides, it appears that the turgidity of the spleen commences when the stomach is emptying itself into the duodenum, not only before the chyle enters the lacteals, but before the chyme is converted into chyle.

The condition of the animal, in the first experiment, is said to have exhibited every mark of plethora or over-fulness of the *vascular* system. In the symptoms noted, however, I can see no indication of any such plethora; but of plethora of the *venous* system the signs are strong.

When the food in the stomach has been reduced to chyme, the stimulus to the secretion of gastric juice ceases, and the nervous influence which effected the secretion is restored to the spleen; the chemical nerves of the spleen thus again acquire a positive condition, and the nerves of its arterial structure an opposite state, hence its arteries permit of distention causing the turgidity of the organ, which commences when the stomach is emptying itself, that is, when digestion in the stomach is completed.

When the spleen is removed, there is no diverticulum to the nervous influence set free when digestion is over, that force of nervous influence, therefore, is determined elsewhere. In the animals experimented on by Mr. Dobson, it was thrown upon the nerves which preside over the involuntary movements. The heart and arteries acquired a contractile power sufficient to confine the circulating fluid almost wholly to the veins. I would say that the condition of the animal (Exp. I.) resembled that of a person in the cold stage of ague. If blood had been taken from an artery whilst the dog was in its nearly torpid state, it would have looked like venous blood, such as that which circulates in an animal during hybernation. Were the secretion of the gastric juice, in animals deprived of the spleen, to cease suddenly, syncope would be the consequence; for, by the sudden and great accession of contractile power, the heart would be thrown into a state of continued contraction for a time; such a state as that which is sometimes produced by a blow on the head. The dilated pupil, in the experiment alluded to, shews that the smaller arteries had their contractile power increased, and the frequent micturition, the contracted state of the bladder.

We find, by the second experiment, that when the animal was fed with small quantities every hour, or every two hours, that no symptoms of uneasiness occurred, and Mr. Dobson accounts for this by supposing, that, owing to the mode of feeding, the increase in the volume of blood was not more than equivalent to what had been previously expended by the secretions. According to my view, however, the cause why no uneasy symptoms appeared is to be found in the constant stimulus to the secretion of gastric juice being kept up; and I have no doubt but that if food had been given to the first dog three hours and a half after it was fed, or before the uneasiness commenced, it would have been warded off, notwithstanding the full meal.

I do not mean to say that, in certain conditions of the system, the spleen does not act as a receptacle for the blood; for there are two states in which its usefulness as a safety-valve is very evident: when there is distention of the arterial system, and when there is congestion of the venous system. When we induce over-fulness of the arteries by violent exercise, the arterial structure of the spleen becomes distended, and that distention is not unfrequently accompanied with pain, because the numerous nervous filaments possessed by the organ are distributed to that structure. That one of the uses of the spleen is to relieve the system, when there is too much blood in the arteries, seems probable from the circumstance that in the horse, whose nature intends and fits him for violent exercise, the splenic artery is not, as in man, furnished from the cœliac, but originates at once from the aorta. When the turgidity of the spleen is caused by venous blood, as in the cold fit of ague, it is unaccompanied by pain, for there is then no stretching of the nerves.

Compared to its size the spleen receives a great number of nerves, this alone proves that its use is not merely that of a diverticulum to the blood; and that these nerves are not all sentient nerves is very evident from the fact, that the sensibility of the organ is very feeble; it seldom suffers from inflammation, and when it does, there is little pain; even when wounded, the pain is trifling.

I do not pretend to say that I have come to any very satisfactory conclusion as to the nature and use of the product secreted by the spleen when its nervous influence is restored after digestion in the stomach is ended; but I adopt the generally received opinion, that it, in some way or other, contributes to the process of sanguification. And there are many circumstances occurring in the healthy and the diseased states of the body, which, taken collectively, lead to a surmise that the blood does in a great measure derive its fibrin from the action of the spleen.

It is generally admitted that the blood of the splenic vein is more fluid and is made to coagulate with greater difficulty than the blood of the splenic artery. Dr. Rush informs us that the blood taken from the spleen is less coagulable than venous blood taken from the arm, and the experiments of Sir Everard Home confirm the truth of the observation. Blumenbach also remarks that the splenic blood is very fluid, coagulates with great difficulty, and separates the serum from the crassamentum imperfectly. From the change which takes place on the blood in the spleen results a fluid of a reddish colour, which Professors Tiedemann and Gmelin say possesses the property of coagulating, and which is carried to the thoracic duct. This product therefore possesses the property which the blood passing from the spleen seems to have lost. The Professors also observe that the chyle in the thoracic ducts of animals deprived of the spleen is whiter and more fluid, that is, it has less concrescible and colouring matter than the chyle of animals in which the spleen is present. Now, if the spleen act according to my notion, as a secretory organ only when the stomach is passive, it ought to follow that where the digestive apparatus is almost always active, as in graminivorous animals, the chyle in the thoracic duct should resemble, in a great degree, that of animals without the spleen: this appears to be the case. Dr. Marcet found the chyle of graminivorous animals thinner and darker, and less charged with albumen than that of carnivorous animals: yet he could discern no difference in the composition of the albuminous portions. The darker hue may easily be accounted for on the supposition that some of the coloured particles of the blood escape undecomposed into the lymphatics of the spleen, and that in greater abundance during the inactivity of the organ whilst the stomach is in operation.

Admit the spleen to be a secretory organ only when the stomach is quiescent, and that its use is then to charge the blood with much of the fibrin found in it, and we can be at no loss to account for the rigidity of muscular fibre acquired by animals which are subjected to long fasting. Animals which are fed but once a day, such as the hound, acquire a greater rigidity of muscular fibre than if the same quantity of food were given in divided portions at intervals; and wild animals, whose fasts are long, acquire a muscular strength, which in many is amazing. The race-horse is amply supplied with oats, but he does not fatten, because, as he is limited in hay, his digestive apparatus is not kept in constant activity; thus his spleen is permitted, by lengthened exercise of its secreting function, to charge the blood with fibrin. To the stall-fed fattening bullock, however rich his food, straw is necessary, for, although there be in it little or no nutrition, it conduces to the formation of fat, as, by prolonging the activity of his stomachs, the spleen cannot operate in charging the blood with fibrin, and, I should think, that as nitrogen is a constituent of fibrin, and not of fat, the blood which contains least of the former must the more readily produce the latter.

The inactivity of the spleen, by withholding fibrin, conduces to the production of fat; therefore the removal of the spleen altogether ought, in like manner, to produce the same effect; and it does so. I have a quotation in my note-book, to which I have unfortunately neglected to add the authority, wherein it is stated that the spleen cannot be very essential to life, inasmuch as animals deprived of it not only live but grow fat. The dog in the second experiment of Mr. Dobson, as before quoted, became fat in three weeks, although the healing process in the wound was going on during the time.

I know not if the spleen of the hog be small compared to the weight of the animal; but there are many circumstances connected with his nature which lead me to think that all the nervous influence which effects the secretion of the gastric juice is not consumed by his spleen, as in other animals, when digestion in the stomach is ended, and that, in some degree, the secretion of that fluid is with him perpetual. It was remarked that the thymus gland in the pig was very large compared to the weight of the animal; so large indeed is it, as to make it probable that a greater force of nervous influence is expended on it than is sufficient for the respiratory apparatus, and many of the anomalous features in the character of the hog, make it not improbable that the surplus nervous influence is, after birth, directed to the stomach. These anomalous features, however, may readily be accounted for on the supposition that the stomach receives at birth a portion of the nervous influence which the thymus held in play. I said that, were it not for the spleen, animals would be incommoded by a constant craving for food, and this is proved to be the case by the voracity remarked in those deprived of it. Now if the stomach, in the hog, receive part of its nervous influence from the thymus, he will in some measure resemble an animal deprived of the spleen; for as that which is derived from the thymus is not restored, the portion of nervous influence so derived must be constantly effective. The hog, therefore, ought again to become edacious as soon as his stomach is emptied.

Buffon says, "of all quadrupeds the hog appears the most rough and unpolished, and the imperfections of his make seem to influence his nature; all his ways are uncouth, all his tastes are unclean, all his sensations are confined to a furious lust, and to a brutal *gluttony*, which makes him devour without distinction every thing that presents itself, and sometimes even his own young when they are first born. His voraciousness apparently depends on the *continual want* which he has to fill the vast capaciousness of his stomach." The continual secretion of gastric fluid gives him the perpetual desire for food, so that, whilst food is to be got, his stomach may be said never to be empty; and as the spleen is found to be small when the stomach is in operation, this constant operation must render the spleen, as a *diverticulum* to nervous influence, less necessary to the hog than to other animals; therefore, in his species, I venture to say, the spleen will be found small compared to the weight of the animal. In the great tendency to produce fat, the hog also resembles an animal deprived of the spleen.

If, from peculiar conformation, the hog is tormented by a constant craving, which forces him to feed that he may be fit to be fed on, nature has also been kind to him for himself; for, that he might always eat whilst there was food, there is bestowed on him the singular benefit of retaining his first teeth. "Other animals, such as the horse, the ass, the ox, the sheep, the goat, the dog, and even man, lose all their first incisive teeth, which fall out before puberty, and are soon replaced by others. In the hog, on the contrary, these teeth never fall out, but continue to grow during life."

The great development of the supra-renal capsules in the human fœtus shews that they are more necessary to its economy than to that of the adult, and I think that whatever be the use of the fluid secreted by them, its production and use are of but little moment compared to the benefit which accrues from their expending in the fœtus the nervous influence destined for the secretion of the urine. In the human fœtus these glands are at least equal in size to the kidneys, and it is necessary that they should be so, that all the nervous influence, afterwards determined to the kidneys, may be expended; for if the urine were secreted in the human fœtus in such quantity as to pass from the bladder, the danger to the mother would be imminent, and so obvious as to require no comment. It perhaps may never be directly proved that the thymus gland is a diverticulum to the nervous influence necessary to respiration, and in some animals, in part, to digestion also; but comparative anatomy comes more immediately to our assistance, in proving almost directly that one use, at least, of the renal capsule in the fœtus, is to hold in play a power destined for a particular purpose after birth. If this appear evident with regard to the capsule, analogy must then become a powerful argument in favour of a like office with regard to the thymus.

It is said, that the great development of the supra-renal capsules in the fœtus shews that their office has more to do with it than with the adult. This is strictly true in reference to the human subject, and, I believe, to the monkey tribe; but in many of the inferior animals it does not hold good, for although there are no warm-blooded animals in which these bodies are wanting, yet they are very diminutive in some compared to the size of their kidneys, and to the relative size of the capsules in the human fœtus. If the size alone of the capsules in the human fœtus lead to the belief that they are more subservient to it than to the adult, it will of course be granted that they perform some office in the human subject which is not required of them in animals in which they are very small. In some they are very small at birth, and do not diminish in after life in any appreciable degree, so that these, I think, can perform no other office in the fœtus than that which they continue to execute after birth, and, if so, such animals cannot be said to possess *fœtal* capsules. Cuvier says that the supra-renal capsule of the lama of a day old he found to be only one-hundredth of the size of the kidney, whereas in the human subject it is equal in bulk to the kidney.

I conceive that the principal use of the capsule in the human subject is to expend the nervous influence meant to effect the secretion of urine after birth; but this office cannot be required of the capsules in the lama, and we know it is not, for he is one of those animals in which urine is secreted *in utero*, and that without danger to the mother, the allantoid being provided to receive it. In the human subject there is no allantoid, but here the capsules being as large as the kidneys themselves, and being largely provided with nerves, are well adapted for the use I have given to them. When the secretion of urine is established, the capsules dwindle, and in the adult it is found that the kidneys exceed them in size by fifteen times. The kid when born has a capsule one-sixteenth the size of the kidney, about the relative size of it in the human adult; still, although it is so much larger than that in the lama, it can be considered to have no other office than that which it executes after birth, if it has, it is not that of diverting nervous influence from the kidney, otherwise an allantoid would not have been provided.

As we find an allantoid where the capsules are small, and none where they are large, it is not unreasonable to conclude that the great development of them in the human subject renders the presence of such a membrane unnecessary; and the only way in which they can render it unnecessary is by diverting that power from the kidneys, which otherwise would produce the fluid the allantoid is furnished to receive.

The existence of an allantoid where the capsules are small, or where, it may truly be said, *fœtal* capsules are wanting, proves, beyond doubt, that their principal use in the human fœtus is that which I have assigned to them; and if a particular contrivance be necessary to consume the comparatively small force of nervous influence afterwards required for the secretion of urine, how much greater must be the necessity for a similar design to consume the immense force of the same influence destined to flow to the respiratory apparatus at birth. The

Thymus Gland in all its bearings declares itself to be that design.

As a further proof of the correctness of my view of the use of the capsule in the human fœtus, I may mention that in those animals from which the escape of urine can neither injure them nor the parent, there is no supra-renal capsule at all; this is the case with fishes.

Alnwick, 1*st January*, 1834.

Printed by W. Davison, Alnwick.